ECONOMIC GEOLOGY

VOL. XVIII. OCTOBER–NOVEMBER, 1923. NO. 7

THE ORIGIN OF METALLIC CONCENTRATIONS BY MAGMATION.[1]

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THE genealogy of our modern thought as to the formation of ore deposits runs back at least to Elie de Beaumont, who recognized the connection of many ore deposits with igneous rocks. De Beaumont wrote in 1847:

> The water vapors, which spring either from the cooling lava, or from the crater-fissures, form sometimes through their condensation, veins of hot water, which contain different salts and are really warm springs. Like the volcanic emanations, they originate from a natural distillation and sublimation.
>
> In general, mineral waters are chiefly found where volcanic eruptions have taken place, or at least in places where the formations are highly altered. But this is exactly the usual occurrence of veins, which are found principally where the formations are disturbed and faulted, and eruptive rocks are near by. The principal difference is that warm springs are associated with the recent eruptives, the veins with the older ones.

De Beaumont's theory of ore deposition began with the detection of metallic deposits at certain fumaroles, and the observation that fumaroles on cooling are transitional into hot springs. Thus de Beaumont early stated a theory of ore deposition which has experienced a revival, especially in America, during the last few decades. It has been often stated that de Beaumont's ex-

[1] Presidential Address delivered before New York Meeting, Society of Economic Geologists, May, 1922.

planation of ore deposits was that they were deposited from gases. Thus Van Hise [2] observes:

> That ore deposits have been precipitated from gaseous solutions has been a favorite hypothesis ever since ores were a serious subject for investigation. This idea goes back as far as Elie de Beaumont, who supposed that the majority of ore deposits were thus produced. This theory has been especially applied to ores of mercury and tin.

But De Beaumont's conception was actually broader and quite modern, for, in submitting that only by logic can we arrive at the facts, he observes,

> One cannot penetrate the interior and demonstrate the connection between the fissure channels and the mineral springs, and show the point where they can extract from the eruptive rocks their heat and their constituents.

This view that volcanism and ore deposition are closely allied phenomena has sometimes, with considerable propriety, been called the French School. It is a line of thought which has been followed and further developed by various other geologists. Sir Charles Lyell adopted this view. He observed in 1857,[3] after calling attention to the circumstance that veins are formed in fissures,

> We also know that mineral veins are most metalliferous near the contact of plutonic and stratified deposits, especially where the former sends veins into the latter, a circumstance which indicates an original proximity of veins, at their inferior extremity, to igneous and heated rocks. It is also ascertained that all the substances with which hot springs are impregnated agree with those discharged in a gaseous form from volcanoes. . . .
>
> Although we have been led to infer from the foregoing reasoning, that there has often been an intimate connection between metalliferous veins and hot springs holding mineral matter in solution, yet we must not on that account expect that the contents of hot springs and mineral veins would be identical.

When I first studied geology and began professional work the prevalent thought in America was rather of what we may call the

[2] Treatise on Metaphorism, page 1053.

[3] "Elementary Geology," New York, p. 626.

German School, which goes back to Bischoff and Sandberger. Bischoff wrote [4] in 1864:

> As a general consequence of the relations between the matrices of lodes, the rocks adjoining them, and their condition, as well as those between different lodes, it may be inferred that all the substances contained in the lodes have been derived from the adjoining rocks.

Bischoff found the materials of vein gangues in the rocks; and when Sandberger in 1882 announced that as a result of his researches he had detected in rocks, especially in the basic silicates of crystalline rocks, all of the metals, the proof seemed to many to be complete that the country rocks were the source of the metalliferous veins found in them. This was the so-called lateral secretion theory of vein formation. Even before the publication of Sandberger's work, James D. Dana wrote in 1879: [5]

> Whenever a fissure is opened in a rock to a great depth, the space thus made is almost like a vacuum as to pressure, compared with the region alongside. Consequently, any materials in the adjacent rock that are vaporizable, under the new conditions, or can be taken up by heated moisture present, or by other solvents, will rush toward the open space. Thus depositions against the walls will be commenced; and new supplies may be kept up until the open spaces fill or the supply gives out. . . . The material filling the fissure may hence come from the country rock adjoining the part in which it is deposited, or from depths much below it, but not, it is believed from areas of fused rocks or ores at the lower extremity of the fissure, as in making dikes.

Subsequently and after the work of Sandberger, Emmons, for example, wrote in 1886 [6] that "the weight of what may be considered actual evidence, as distinguished from pure speculation, seems to be in favor of the lateral-secretion theory in its broader acceptation." In 1893 the Austrian geologist Posepny read a paper before the American Institute of Mining Engineers in which he directly opposed the lateral-secretion theory and appealed to ascending hot waters as the agents which had formed ore deposits, the waters having derived their contents from

[4] "Chemical and Physical Geology," III., p. 548.
[5] "Manual of Geology," 3d ed., p. 774.
[6] Monograph XII., U. S. Geol. Surv., Part II., p. 575.

depths greater than the rocks which we know—from a "barysphere" rich in metallic minerals, following in part the conceptions of De Launay.[7] Posepny thus led the reaction, in America, again the German School, and back toward the French School, which had just been ably reviewed by De Launay, in France. This essay of Posepny's was largely instrumental in breaking up the adherence to the lateral-secretion theory in the United States. But Van Hise, as late as 1904, wrote:[8]

> It is therefore clear that the gangue minerals are largely segregated from the immediately adjacent rocks traversed by the veins. If this be so for the dominant minerals, there is every reason to believe that the same conclusion applies to the very subordinate amounts of valuable metals which constitute a small or almost an inappreciable part of the filling of an ore deposit.

In fine, we may discern two main lines of thought as to the origin of, let us say, fissure veins—in order to concentrate our thought on a certain clear type of ore deposit—first, the earlier philosophy of De Beaumont and Lyell, based upon field observation of the relations of volcanoes, igneous rocks, fumaroles, and hot springs, and the metallic and non-metallic precipitations from fumaroles and related hot springs; and, second, the later laboratory-born chemical theory of Bischoff and Sandberger, based upon comparative chemical research, which showed that the constituents of fissure veins were often identical with those found in the rocks which they traversed. The older theory did not take into account, as an essential factor, the composition of the rocks; and the later chemical theory took little heed of actual field phenomena, but, nevertheless, prevailed in America for ten or fifteen years over the volcanic theory. The reaction of Posepny against lateral secretion was a definite one, but he did not go all the way back to De Beaumont and Lyell: he did not recognize volcanicity or igneous activity as an essential to ore deposition. although he did point out that most ore deposits were in regions of eruptive rocks. But directly after Posepny's paper, a younger

[7] *Am. Inst. Min. Eng.*, Chicago meeting, 1893. "The Genesis of Ore Deposits," p. 67.

[8] Monograph XLVII., U. S. Geol. Surv., p. 1071.

American School arose, which, making use of field observations and consequent inductive reasoning, worked essentially back to the school of De Beaumont and Lyell. Of this school were Kemp, Weed, Lindgren, and myself, and, later, many others; and many important papers which I will not try to enumerate built up an impressive array of facts and conclusions. The source of mineralizing waters was more and more referred to the igneous magmas, as De Beaumont and others, including the Austrian geologist Suess, had done: in other words, such waters were not conceived of as surface-derived or atmospheric water, but as largely new water, born from the crystallizing igneous rocks. The lateral-secretion theory was by no means eliminated from the field, but more and more driven into corners, especially into the field of the lead and zinc deposits of the Mississippi Valley, where the scarcity of igneous rocks made geologists loath to deduce igneous ore solutions as the source of the sulphides. Some of us, indeed, conservatively wavered between the magmatic theory and the lateral-secretion theory, assuming that some ore deposits were formed by one, some by the other, method; and that some again were formed by a combination of the two. I myself was among those who thus hesitated for a number of years, seeming to find varying testimony in different fields of ore deposition.

Posepny had done a service in calling our attention to the difference between the shallow or vadose waters and the waters which rose from great depths, to the latter of which he referred primary ore deposits. The importance of the shallow or clearly atmospheric waters in working over and concentrating and enriching ore deposits was pointed out by Penrose in 1894,[9] who wrote:

> As a result of these various changes, certain materials are sometimes leached from the upper parts of ore deposits, which have become porous by alteration, and carried down to the less previous unaltered parts. Here they are precipitated by meeting other solutions, or in other ways, and hence the richest bodies of ore in a deposit often occur between the overlying altered part and the underlying unaltered part. This is not always the case, but it is true of some copper, silver, iron and other deposits.

[9] R. A. F. Penrose, Jr., *Jour. Geol.*, Vol. II., 1894, pp. 288–317.

In 1891 Dr. James Douglas had described such enrichment of copper in Butte, saying that "it seems as if the copper, leached out of the 400 feet of depleted vein, had been concentrated in the underlying ore, and had thus produced a zone of secondary ore about 200 feet deep, which contains, as might be expected, about thrice its normal content." [10]

These principles were brought out forcibly and at length by Emmons, Weed, and Van Hise in 1900. A great deal of later work has been added which proves the importance of atmospheric waters in reworking primary ores; and in producing residual deposits, like those of iron and manganese, from rocks containing them. But the formation of sharply defined primary ore deposits, such as fissure veins, by widely circulating ground waters, drawing disseminated metals from the rocks into fissures, has received no confirmation from these studies; and this lack of confirmation has strengthened the theory of magmatic origin for such deposits.

The two opposing schools of thought, which we may call the lateral-secretion school and the hot-spring school, both ascribed the deposition of ores to the agency of circulating waters, whether atmospheric or juvenile. Sharply defined from both these opposing theories is the theory of magmatic differentiation, first proposed, so far as I know, by J. H. L. Vogt, of Christiania, in 1893.[11] The idea of magmatic differentiation of rocks dates as far back as Scrope in 1825. Scrope, and later Dana, Darwin, and others, recognized a definite order or succession in lavas, which they ascribed to some process of segregation in the underlying mother magma. When the advent of microscopic petrography lent impetus to igneous rock studies, the problems of magmatic differentiation were dwelt on by Iddings, Brögger, Loewinson-Lessing, and many others. "It has come to be generally accepted," wrote Iddings in 1914,[12] "that the various kinds of igneous rocks belonging to one period of volcanic erup-

[10] *Trans. A. I. M. E.*, Vol. XIX., 1891, p. 693.

[11] "Bildung von Erzlagerstätten durch Differentiationsprozesse in basischen Eruptivmagmata," *Zeit f. prakt. Geol.*, 1893.

[12] "The Problem of Volcanism," p. 111.

tion in any one region have been derived from some parent magma, by processes of differentiation that have caused the concentration of some of the mineral compounds in parts of the magma that have flowed at different times, and have solidified into different kinds of rocks. There are various opinions as to how such separations have taken place, but a very general agreement among petrologists that they have done so." Brögger, between 1886 and 1898, pointed out the parallelism between the sequence of crystallization and that of differentiation. By such processes of differentiation, it has come to be agreed, not only have different lavas originated, but different plutonic and dike rocks; and in many of the plutonic rocks the more or less arrested stages of differentiation may be studied; and this differentiation or segregation, chemical and therefore mineralogical, may be studied at times even on so small a scale as a single hand specimen.

Taken altogether, this recognized geological fact of magmatic differentiation is relatively as important as erosion, or sedimentation, or intrusion, or any of the recognized major geological processes. Yet in a few standard text books of geology which I took up casually—namely Pirsson and Schuchert's, and Grabau's—I find no reference to that process. It may be there, but I did not find it; and it is not listed in the indexes. Nor is it quite familiar to the thought and speech of geologists. One reason is possibly the complex nomenclature, which is formidable—*magmatic differentiation.* I make a tentative suggestion to abbreviate magmatic differentiation and magmatic segregation, which I understand to mean the same thing, to ***magmation,*** and so try to eliminate the nomenclature handicap. Another obstacle has been a lack of comprehension of the workings of magmation. Molecular diffusion was embraced and discarded; fractional crystallization recognized, but found insufficient except in small areas; thermal-convection currents were appealed to, but half-heartedly. Gravitative action, the settling of earlier-formed crystals in a still liquid residual magma, has more recently been appealed to as explaining cases of large-scale differentiation; but

it fails to explain other cases equally as important. Finally, there is the principle of gaseous transfer, of which Daly [13] writes:

> Mere gravitative differentiation in liquid magmas cannot explain certain small basic or ultra-basic phases in intrusive bodies. Contact segregations of magnetite, ilmenite, mica, hornblende, tourmaline, etc., are often found. There is growing belief that these are generally due to upward transfer by emanating gases.

I am inclined to suspect that this gaseous-tension differentiation is more important than thus indicated, and that it may be the chief process of magmation.

Vogt explained certain ores as due to this process of magmation, therein, as I said before, establishing a markedly different new school of thought in the explanation of ore deposition. Instead of water, whether atmospheric or expelled from cooling magmas, as hot springs or fumaroles, Vogt appealed to the igneous magma itself as the carrier and the segregator of ores; and thus water and magma as agents of ore concentration are clearly set off from each other. The question then comes up: What is magma? We know it is the fluid from which igneous rocks crystallize; but we now recognize it as also the source of certain concentrations of metallic minerals; and it is also the source of much water, and of gases of many kinds. Unfortunately, we cannot study magma; even the lavas, as they are poured out, are no longer in a truly magma condition, having parted with and being in the process of parting with the gaseous constituents which are an essential part of the definition of magma: we can only reconstruct magma, in our conception, inductively. Experimental work in the laboratory has thrown light on the subject by the reproduction of many magmatic minerals, but has been unable to reproduce magma, chiefly on account of the high pressures that would be required. Experimental reproduction of a granite still lies outside the achievement of the physico-chemist.

Vogt wrote: [14]

[13] "Igneous Rocks and Their Origin," New York, 1914, p. 247.

[14] "Ore Deposits." Beyschlag, Vogt and Krusch, Truscott's Translation. Macmillan, London, 1914, p. 243.

With most eruptive rocks crystallization first begins with what may be called the ore-minerals, such as magnetite, specularite, ilmenite, zircon, apatite, pyrite, and occasionally also spinel, etc. At a somewhat later stage of consolidation, the ferro-magnesium silicates, such as mica, hornblende, and pyroxene minerals, and olivine crystallize. It is in agreement with this sequence that, apart from some exceptions, just these components are subject to migration in magmatic differentiation.

"Magmatic segregations," says Vogt again, "according to their content are divided into oxide, sulphide, and metal deposits." Under magmatic segregations Vogt describes chromite deposits, corundum, titaniferous iron, apatite, nickel-pyrrhotite ores, and certain "intrusive pyritic sulphide deposits," which are often low-grade copper ores, containing also some galena, blende, etc.; also native nickeliferous iron, and platinum.

I now wish to call especial attention to the following quotation from Vogt:[15]

In addition to these magmatic segregations the pneumatolytic and the contact-metamorphic deposits must also be considered, both in genesis and in space, as occurring in close relation to eruptive magmas. There is this difference, however, that while the magmatic segregations result from a single process, that of magmatic differentiation, in the case of the other deposits the metal after such differentiation was taken up in aqueous or gaseous solution and deposited later by other processes. It is natural, therefore, that in the majority of cases the magmatic deposits are quite distinct from the related pneumatolytic and contact-metamorphic deposits.

I quote this last not because I agree with the deduction made, but because I disagree; to show that while I range myself as to the explanation of ore deposition, with Vogt, as essentially a pure magmationist (as I shall explain), yet I differ sharply from him, as I will show, in his distinction between magmatic segregations on the one hand, and pneumatolytic and contact-metamorphic deposits on the other. And as a preliminary toward attacking the question from another and my own individual angle, I will note that as to gold-quartz veins, of which Vogt had apparently seen little, he writes that the genesis of the pegmatitic

[15] *Op. cit.*, p. 243.

quartz lode has not yet been satisfactorily settled (pp. 188, 346). Indeed, he states (p. 601) that "The connection between these lodes ["the old gold lodes"] and eruptive magmas is not so close as is the case with the young gold-silver lodes."

The magmatic segregations of Vogt are evidently considered as dry segregations, since he eliminates gaseous agencies, the chief of which is water; and he admits as magmatic segregations almost exclusively ores connected with basic rocks, considering the ores as among the first minerals to crystallize,—according to the petrographer's conclusions, arising from the frequently clean-cut or idiomorphic form of these ore minerals, where they occur in the igneous rocks; and considering that these earliest-formed minerals have migrated, by a process which he does not satisfactorily explain, so as to concentrate.

My own views as a self-styled magmationist, therefore, find no support in the views of the founder of the magmationist school. While Vogt found his inspiration in the extremely basic rocks, I have always been fascinated by the extremely siliceous igneous rocks—granites, quartz-feldspar rocks or alaskites, pegmatites, quartz dikes, and related quartz veins. Certainly, however, these siliceous extremes are as truly magmatic as the basic extremes; and when we recall that even in granites it is universally acknowledged that water and other gases are essential magmatic ingredients, without which there would be no granite magma, it is realized that Vogt has erred in excluding the agency of gases from the processes of magmatic differentiation. Daly notes[16] that the dominance of hornblende and mica among the femic constituents of the syenites, "shows, in general, the presence of magmatic water in quantity at least ample enough to insure the crystallization of the minerals"; and even as regards certain basic igneous rocks, such as hornblendites, he truly observes:[17]

> It may be recalled that the experimental formation of amphibole has never succeeded if water is absent from the melt. . . . There can be little doubt that these segregations are due to gaseous or vapor transfer, and

[16] *Op. cit.*, p. 401.

[17] *Op. cit.*, p. 454.

that such cases tend to corroborate the view that water gas and other volatile substances have coöperated in the formation of certain hornblende rocks which have been more directly derived from magmas.

Starting from this new angle—that of the extremely siliceous igneous rocks—I stated in 1898 that certain gold-quartz veins of Alaska were in my opinion the end product of magmatic differentiation, since I had traced the transition from granites to pegmatites, and from pegmatites to gold-quartz veins. This explanation aroused interest, but not immediate approval. In the little book on "Ore Deposits—a Discussion," published by the *Engineering and Mining Journal* in 1903, Mr. T. A. Rickard in his preface states that

> Mr. J. E. Spurr in his examination of the Yukon district in Alaska had become impressed with the evidence of ore segregation afforded by a series of closely related rocks in the Forty Mile region, and in his report, published three years ago, he announced a radical departure from accepted views by describing the gold-quartz veins of the Yukon as the end product of rock segregation. . . . Mr. Spurr holds that certain gold-bearing quartz veins in the Yukon have originated by a process of magmatic segregation, that they represent merely the siliceous extreme of the process, the final stage of which is marked by a magma so attentuated as to be described as highly heated water heavily charged with silica and other mineral matter including gold.

As I look over the comments of many of my contemporaries on this theory, in this little volume, I get the distinct impression of sadness rather than of joy at the announcement. Van Hise in his "Treatise on Metamorphism" weighed the theory and criticized it acutely and in general fairly. He observes:[18]

> If the exceedingly rare metal, gold, be segregated by magmatic processes to such an amazing extent as to produce an ore deposit, it seems to me exceedingly likely that other metals would be segregated in connection with the gold ores so as to be present in greater amounts than usual. Since Spurr gives no certain criteria by which gold-quartz veins supposed to be produced by magmatic segregation are to be discriminated from those formed by solutions, I take the conservative view that gold-quartz veins have their origin in solutions, and I ask for positive evidence from those who assert that such quartz veins are produced by magmatic segregation alone.

[18] Mono. XLVII., U. S. Geol. Surv., p. 1049.

My only objection to this criticism would have been to point out that according to my theory also gold-quartz veins do "have their origin in solutions" and that I had already given positive evidence that the quartz veins in question were "produced by magmatic segregation alone." My theory was an inductive one, the result of field observations.

I was able in 1905, through a study of the gold-quartz veins of Silver Peak, in Nevada, to demonstrate this theory by much more detailed observations; and here I advanced in thought, in that instead of considering the gold-quartz magma practically water, as I had stated in summarizing my theory for Alaska, I decided that the gold-quartz magma of Silver Peak had been a highly concentrated one. I stated [19] that the sedimentary rocks had been intruded by a highly siliceous alkaline magma, which crystallized principally as feldspar and quartz. Concerning the crystallization, I observed, "It was, however, so slow that the residual viscous quartz, before its final consolidation, was in part drawn off into large and small reservoirs, and so could play the rôle of an independent intrusion." As a matter of fact, my visualizing the quartz magma, or occasionally gold-quartz magma, in my original proposal for the Alaskan occurrences, as practically water, had been merely an inheritance from the fixed belief of myself and others that waters were the universal ore solutions; but in Silver Peak closer reasoning eliminated this preconception.

Still the objections urged by Van Hise held good, namely that I had not shown satisfactory criteria between gold-quartz veins which were extreme magma types, and those formed by waters: and his further objection that if gold were thus magmatically concentrated, then the other metals should be concentrated also. And I finally found the answer, and proposed it in 1907 [20] as what I call the Zonal Theory of ore deposition. This theory assumed that in the residual magma from which, for example, pegmatites and later gold-quartz veins were formed, the other

[19] Professional Paper 55, U. S. Geol. Surv., p. 112.

[20] Econ. Geol., Vol. II., No. 8, 1907, pp. 781–795.

metals were also contained, and that these passed into the magma still residual from the consolidation of the gold-quartz vein, and were successively deposited, each at a separate and critical temperature of freezing, with the result that the different metals were ideally deposited one above the other, or one outside the other, with falling temperature, and thus ores of copper, zinc, lead, silver, and other metals were successively deposited. "Summing up," I wrote "this theory proposes that metalliferous fluids, from which most ore deposits are precipitated, are extreme differentiation phases of rock magmas. . . . and that the most striking chemical difference between ore deposits is due (in the more important class representing the siliceous extreme) to successive precipitation in theoretically vertical zones, as the fluid migrates toward the surface, and with diminishing heat achieves more and more mature crystallization." This theory therefore postulated an original magmatic solution containing within it all the metals represented in the whole vein sequence, which were successively precipitated as the solutions moved further and further away from their point of origin, and as temperature and pressure grew less.

If my first proposal of the magma origin of gold-quartz veins was received, as I above noted, more with sadness than with joy, my final triumphant solution of the problem as contained in the Zonal Theory was received in many quarters more in anger than in sorrow. One prominent geologist wrote me expressing his regret that I should have written this paper; another (thinking in print) imagined some depravity of character to be indicated; and still another thought it extremely funny, and chortled over it in a contribution to the *Mining and Scientific Press*. A prominent engineer told my partner, Mr. Cox, that if I wrote such eccentric stuff, no one would give me a job. Abroad, however, the proposal was better received. Now, after fifteen years the Zonal Theory is in a fair way of being generally accepted. It explains many things satisfactorily and has stood the test of time. It has been adopted by many geologists in their writings; and Professor Kemp, in his presidential address before the Ge-

ological Society of America in 1922, quite properly referred to my original paper as follows:

> In 1907 Spurr published his valuable contribution "A Theory of Ore Deposition," in which he outlines the theory of ore deposition in successive zones or stages, away from a central magmatic source, the different zones being marked by characteristic metals. This sequence has been corroborated in whole or in part by many observers.

The origin of gold-quartz ores as a siliceous extreme of magmatic differentiation has long since been corroborated by so many geologists that it would indeed be a difficult task to marshal the references.

In 1916 I discovered that my original theory proposed in 1898 for the gold-quartz veins of Alaska had long been anticipated by Thomas Belt, who wrote in 1861 and again in 1873. In his 1873 book [21] Belt wrote:

> There is also sometimes a complete gradation from veins of perfectly crystallized granite through others abounding in quartz at the expense of the other constituents, up to veins filled with pure quartz. . . . I think the probability is great that quartz veins have been filled in the same manner—that if dikes and veins of granite have been an igneous injection, so have those of quartz. . . . I know that this opinion is contrary to that usually held by geologists, the theory generally accepted being that mineral veins have been produced by deposits from hot springs; but during twenty years I have been engaged in auriferous quartz-mining in various parts of the world, and nowhere have I met with lodes the phenomena of which could be explained on this hypothesis.

I was glad indeed to find that my conclusion had been anticipated by a man who as a naturalist was looked upon with great respect by Darwin; and to be able to name Thomas Belt, as I do now publicly for the first time, as the first proponent of the magma-origin of gold-quartz veins.

Is everything, then, settled as to the origin and nature of ore magmas—at least, if you so desire to put it, in my own mind? Not yet. The mind moves slowly away from notions early instilled. I noted above that in proposing the origin of the gold-quartz veins of Alaska, I pictured the gold-quartz magma as little

[21] "The Naturalist in Nicaragua," republished in Every Man's Library; p. 76.

more than highly heated water, for no reason except that I evidently could not get away from the doctrine which I had been taught, that ores were deposited from waters. I did get away from this preconception when I studied the Silver Peak veins, and as a result pictured the magma as viscous and the gold-quartz as intrusive, much as igneous rocks are intrusive. I stated this in 1905 [22] and, as above stated, Belt had come to the same conclusions as early as 1861. There the matter has rested. When I proposed the Zonal Theory I still stuck to the old assumption of hot waters as magmatic agents, and while granting that some gold-quartz magmas might have been dense and concentrated, I pictured the residues, which deposited copper, zinc, lead, silver, and the like as essentially waters. Recent studies and reflections have induced me to reconsider even this assumption, however; and without here going into the details I may say that I find evidence of many veins of copper, zinc, lead, and the like having been deposited from highly concentrated magmas, which appear in many cases to have intruded the rocks under pressure, much as does an igneous dike. I have tried to describe the evidence and the deductions in a book which I have just published on "The Ore Magmas." I had intended that the present address should be an abstract of the conclusions contained in this work, but I find that I have taken up so much space in my preliminary review that I shall have to pass over most sketchily the new proposals which I wished to submit. Still, I shall impose on you a little more, for it would not do to leave Hamlet altogether out of the play.

To begin with, for those veins such as have been described by Belt, Vogt, and myself, as well as many others, which have been intruded as a thick magma, I propose the name veindikes, for all veins have not been so formed, some having originated from thinner although I believe generally highly concentrated solutions, and having been formed by replacement rather than intrusion.

In the differentiation, or, let us say, magmation of granite and other igneous rocks, among the last highly differentiated phases

[22] *Trans. A. I. M. E.*, 1905, Vol. XXXVI., pp. 960–961.

result aplites and pegmatites. These are found to be nearly contemporaneous with one another, and it is often noted that one has about the same mineralogical and chemical composition as the other; but they are sharply different as to texture, and in cases which I have carefully observed, the average diameter of the average crystal in the pegmatite dike, is, let us say, fifty times greater than that of the average crystal in the aplite dike fabric. Transitions between pegmatites and aplites are not the commonest condition; they are typically strongly contrasted, even though contemporaneous dikes. And this I assume means that the pegmatite magma was relatively dilute and aqueous, the aplite magma relatively dry, and I reason that from the extreme differentiated siliceous product of a granite magma, for example, a magmatic splitting takes place into a relatively dry aplitic submagma and a relatively aqueous or pegmatitic submagma, which submagmas migrate separately and are separately injected as dikes. But even the pegmatitic magma is far different from highly heated water; it is still a true magma, though aqueous; it is still something with which we are physically unfamiliar, and which we have been unable to produce experimentally. I further conclude that the ore magmas which I have postulated under the Zonal Theory of ore deposition have also the habit of separating, at any stage, into relatively dry and relatively aqueous fluids, which each migrate independently; that this is true of gold magmas, copper magmas, zinc magmas, and so on. The metallic magma of the aplitic type acts as an intrusive, and forms a veindike—a copper veindike, or copper vein, for example; the corresponding copper magma of the pegmatitic type acts more effectively by replacement and forms a replacement fissure vein, or an irregularly shaped replacement, or an impregnation, or dissemination. Yet even the aqueous or pegmatitic vein magmas, according to my conclusions, are relatively highly concentrated metallic solutions, and are quite different from the mineralizing waters which I have hitherto pictured as the ore carriers in the case of copper, lead, zinc, and such metals.

Among the criteria by which may be recognized veindikes con-

taining any metal, which veindikes have crystallized from the aplitic phase of ore magmas, are, first, the frequently great width of fissure veins, so great that no previously existing fissure can possibly be believed in. There are veins of quartz and of calcite, with various admixtures of sulphides up to nearly the pure sulphide state, which run up in width to 100 feet or even more; and these widths are maintained between parallel and sharply defined walls, just as they are in the case of an ordinary igneous dike. These veindikes, it goes without saying, are those which do not show evidence of being the work of replacement; indeed, the evidence is positive, showing that they did not form by replacement, as is indicated by the clearcut walls and inclusions of country rock. And this leads to the second criterion of the veindike, which I regard as possibly the most important—the inclusions of country rock, frequently sharply angular and therefore relatively unattacked corrosively by the ore magma, which are commonly found in these veindikes, isolated and unsupported, showing that they floated free in the magma from which the veindike crystallized. These angular inclusions are among the commonest phenomena of fissure veins; and it is an eloquent tribute to the fundamental stupidity of all of us that for so many decades we have failed to read the mute but unmistakable testimony which they offer. Some of the best cases which I illustrate in my book are from my own drawings, of which I failed utterly to recognize the meaning. Since ores were deposited from water, in accordance with the creed which I had been taught, and which I accepted on that faith which is stronger than all reason, then the explanation of all phenomena had to comply with this cardinal element of faith. Therefore I explained these inclusions, as have many others, as representing fragments which had fallen into a fissure—producing what I called a rubble-filled fissure; and if there were no evident points of contact in a given exposure, between the fragments, I assumed that the points of contact were behind the scenes. Since many such inclusions are far removed from other inclusions or from the wall rock, the supposititious points of contact are indeed so far

behind the scenes as to be quite off the stage. Therefore another explanation has been given by some geologists: that of the formation of fissure veins by the force of crystallization; and these detached fragments have been cited as evidence that the fissure could not have existed previous to the vein crystallization. With this last contention I agree, but that the fissure was opened by the force of crystallization is a theory that I think cannot in most cases be satisfactorily substantiated, for example for those veins which are homogeneous and without structure parallel to the walls. The fissures were opened, I believe, by pressure, only not that of crystallization, but that of the unconsolidated ore magma. Moreover, examples are frequent where these detached fragments in a vein are surrounded by successive rings of vein material, and there are corresponding layers parallel to the walls; and here we have a criterion which is not available when the vein-stuff is of homogeneous or massive crystallization, as it so frequently is, and one which demonstrates the fact that the fragments floated free in the crystallizing magma. The weight of this criterion has been missed because banding or ringed structure of this type has been supposed to prove gradual accretion on the walls of an open fissure. Reflection shows that this belief has no firm foundation; the phenomena point to accretion, in such cases as I speak of, on the walls of a fissure, filled and distended by the ore magma. We may occasionally find such ringed banding in igneous rocks—for example, in certain granites and in the orbicular diorites. In igneous rocks, feldspar phenocrysts which have been built up by zonal growth, as they floated free in the rock magma, are common. Further than all this, in many cases I find that the included angular fragments in the fissure veins are not of the same material as the immediately adjacent wall rock, having been brought along from a somewhat different location; and this circumstance quite disproves the origin of the fissure by the force of crystallization.

A third criterion of the aplitic ore-vein or veindike is the behavior of veins to limestone walls. Whereas certain ore solutions have replaced limestone, and thus formed great irregular

deposits, I find others which have not so replaced limestone to any important degree, when brought in contact with it. All the important fissure veins of the Terneras group at Velardeña in Mexico are strong and productive in the diabase intrusion in which they lie, but dwindle, branch, and die in passing into surrounding limestone, though still keeping in the limestone their regular form of fissure veins and their sharp walls. On the other hand, at Matehuala, in Mexico, slightly productive fissure veins in the monzonite intrusion expand into highly productive pipes and replacement masses on passing across the contact into the older limestone. The former instance represents, according to my conclusions, a relatively dry or aplitic ore magma, the latter a relatively aqueous or pegmatitic ore magma. Yet even the pegmatitic magmas I visualize as on the average hardly if at all more aqueous than granitic pegmatites, as having been injected at a definite magmatic stage, and to be by no means confused with hot waters, the instrumentality of ore deposition which we have always postulated. Hot magmatic or juvenile waters, according to my conclusions, are quite another thing. They are, to be sure, given off in quantity from cooling lavas at the surface, and along the contacts of cooling plutonic rocks. I believe similarly that they are given off not only from crystallizing dikes, but from crystallizing veindikes, whether of pegmatite, gold-quartz, metalliferous ores, or that barren calcite veindike stage which I believe to be the last stage of the magmas proper and to be cooled and crystallized at the lowest temperature. With such magmatic waters, indeed, I am inclined to believe are associated at times mercury and antimony, and to believe that the main deposits of these metals are from solutions or vapors which have escaped from the ore magmas, in common with water and other gases, since the high volatility of all these substances prevented their retention at the crystallizing stage of any of the ore magmas proper. The cinnabar and stibnite deposits, therefore, in my opinion, may form a class apart from the general run of metalliferous ores.

The principal reason, perhaps, that the ore magmas proper, as

I imagine and describe them, have not been recognized as an extreme but truly igneous type of magma, is that, unlike rock magmas, they do not emerge at the surface, and therefore, as magmas, they have never been seen. This is equally true, I may point out, for pegmatite magmas, and, I believe, for the same reason. What is this reason? Ore magmas approach near the surface: we have many rich ore deposits which have been formed up to say 500 feet of the surface, but they never emerge at the surface. The fact that they do not indicates to me that they cannot. The necessary depth of at least 500 feet would seem to indicate a critical pressure factor, and that at less than that critical pressure ore magmas cannot exist, but disintegrate, with release of gaseous constituents and abandonment of gaseous conditions. Thus at the upper pressure limit, rather less than a thousand feet from the surface, very rich ore deposits are formed, many of which have a limited vertical extent, for the reason above noted. But the magmatic hot waters, including those which deposit cinnabar and stibnite, do apparently reach the surface as hot springs.

This is as much of a sketch as it is possible for me to draw today. I may perhaps add that I started, years ago, with the belief that the class of magmatic segregation proposed by Vogt did represent, as he believes, the first crystallizations from the igneous magma, and that the gold-quartz veins represented the last crystallizations. Therefore, as contrasted with the basic magmatic segregations of Vogt, I called the gold-quartz veins, siliceous magmatic segregations, leaving them separated by the whole main intermediate process of magmatic crystallization. But following the demonstrations of the Zonal Theory, I find that not only are the different metallic sulphide depositions closely related to the gold-quartz veins, but that they, on the other hand, merge in and are confounded with certain supposedly basic magmatic segregations, as, for example, in the case of pyritic copper sulphide deposits. I find it, therefore, difficult to maintain my old classifications and am driven to merge ore deposits—whether associated with basic rocks or with siliceous rocks—for the pur-

poses of primary classification, into a single category; and to believe that the copper deposits associated with basic rocks have the same general magmatic history as those associated with granites. I question, indeed, the whole assumption that the metallic minerals—oxides, sulphides, and native metals—are necessarily or even generally among the first formed crystals from the igneous magma, as the petrographers have concluded, on which assumption Vogt's view of basic magmatic segregation is founded; on the contrary, I suspect that the metallic minerals, whether oxides or sulphides, are later than the rock silicates in general, and that the idiomorphic form of these oxides and sulphides, on which their greater age has been predicated, may be due to a special adaptability for migration, penetration, and replacement, which is characteristic of all the ore minerals. Thus there may result, according to my view, the various forms of metallic mineral deposition: isolated crystals of metallic oxides and sulphides disseminated in the igneous rock fabric; streaks or bands where these crystals are relatively concentrated; accumulations at igneous contacts, in the intruded rock and also in the intrusive rock; accumulations along and in fissures; and, under favorable circumstances, the accumulation of concentrated ore magmas, which act as individual submagmas, performing intrusion, and subject to successive migration, and splitting into more and more specialized types of submagmas. And I believe that these principles apply to all rocks, whether basic, siliceous or intermediate. To be sure, the type of differentiation or magmation which I assume, and whose impelling force seems to have qualities analogous to those of gaseous tension, can only segregate from an initial general rock magma those elements which are in it; and it is indubitable that such general rock magmas are of different composition, not only as to non-metallic elements but as to metallic elements as well. Nickel, for example, I do not need to restate, is practically peculiar to basic magmas and tin and tungsten to siliceous magmas; but copper, lead, zinc, and iron, and certain other metals, are common to all magmas; and the marginal segre-

gations of copper and iron of basic rocks have the same origin, I believe, as those on the margin of granites.

In conclusion, I wish to philosophize a little on conservatism. Many geologists point with pride to their conservatism. They should not; they should be ashamed of it. A scientist who is conservative is false to his science. Conservatism means holding fast to that which you have. It is a necessary popular virtue. The average man on the street thus holds fast to the principles laid down for him by great thinkers; if he were to let go of them he would be without sail or rudder, for he is unable to think for himself, or very slightly. But the scientist, if he be a true scientist, must be continually advancing in knowledge; he must be continually finding out new and undiscovered things, which alter the aspect of all that has been believed, and these new truths he must be ready to apprehend and embrace. He must not be a conservative, but a progressive. Behind him will follow a great mass of the laity and the semi-laity, who will convert his radicalism and progressiveness into one of the tenets of their conservatism, with perhaps an intervening period of shock and indignation over what, before they assimilate it, appears to be heresy. I do not mean by this to insist that I am right in what I have just sketched out; but to submit that no geologist should think it meritorious if he hold to an old view simply because it *is* old.

Nor do I wish to be misunderstood as stressing views because they are my own. I have shifted with real relief and pleasure my gold-quartz vein theory, after twenty years of carrying it, back to the earlier proponent, Thomas Belt. It may well be that I have overlooked views that forestall and duplicate those newer ones which I have sketched today.

THE EÖTVOS TORSION BALANCE AND ITS APPLICATION TO THE FINDING OF MINERAL DEPOSITS.

STEPHEN RYBÁR.

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INTRODUCTION.

THE researches of Professor Lorand Eötvös on gravity were followed with interest by men of science for many years before the world war, and leading men of physical science frequently visited his laboratories to become familiar with his methods of research. The interest in his work was at first purely scientific. However, since the Hungarian government began to use Eötvös's method in locating salt deposits lying beneath the surface, and especially since the d'Arcy Exploration Company is using this method in searching for petroleum and natural gas in Hungary, Eötvös's method has interested not only men of science but also those interested in the practical application of the method to mineral investigations in determining the geological formations beneath the surface.

Eötvös has developed his method by means of the Potential Theory. In order to thoroughly understand this method it is obviously necessary to be acquainted with higher mathematics. The essence of these researches, however, can be readily understood without presupposing any higher mathematical knowledge, but just a review of fundamentals is necessary.

GRAVITY.

It is understood that a body resting upon a surface generates some sort of a force upon it, and if we remove this surface, it falls in a vertical direction with constant acceleration (in Budapest about 981 cm./sec.^{2}) The force exerted by the body upon the horizontal surface, or the force which acts on the falling body in a vacuum, is what is known as the weight of the body, or the force of gravitation. So far as our experience proves, the attraction of bodies is directly proportional to their masses; hence the force of gravitation can be established by the force acting upon the unit of 1 gram, which is called gravity. Gravity is numerically equal to the acceleration produced by the falling body in a vacuum.

Gravity is found everywhere—on the surface of the earth, high up in the air, or deep underground. The determination of the direction and the magnitude of gravity is the great problem of geodesy. The direction of gravity can be ascertained by a plumb-bob or a water level, and the magnitude with a pendulum, or by comparison with other constant forces.

Eötvös has enriched our knowlege of gravity in large measure by the application of his new method, with which it is possible to determine the difference of gravity within a distance of a very few cm., and also by the accurate determination of the relations of the curve of the niveau surface of gravity.

According to Newton's Mechanics, gravity may be regarded as composed of two forces, the *attraction of the earth* and the *centrifugal force* produced by the rotation of the earth. The direction and the magnitude of the attraction of the masses is determined by Newton's law. Acording to this law, the direction of the mutual attraction of the two particles is in a straight line from one to the other, and its magnitude is directly proportional to their masses (m_1, m_2) and inversely proportional to the square of the distance (r) between the two particles. That is:

$$P = f \frac{m_1 m_2}{r^2},$$

f is an independent constant with relation to masses and their materials, and is called the gravitation constant. The gravitation constant is a matter of physical consequence. That is, if $m_1 = m_2 = 1$, and $r = 1$, then $f = P$; this means that the gravitation constant is in the absolute (C.G.S.) unit of measurement—the force which one gram of mass exerts upon another gram of mass at a distance of one cm.

The value of the gravitation constant in accordance with Eötvös's measurements is: $f = 0.0000000663$ C.G.S.

In this numerical value, force is expressed in the absolute unit, by dynes. It is not difficult to transform this value to a practical unit. The practical unit of force is the weight of a gram, which is in Budapest 981 times greater than a dyne. Therefore the value of this force expressed in the unit of one gram weight is equal to the 981st part of the above number, or

$$f = 0.000000000068 \text{ C. gram weight S.,}$$

which is $\frac{1}{15{,}000{,}000{,}000}$ part of the weight of a gram.

Every particle of the earth exerts, according to Newton's law, a force on a mass outside the earth's surface, and the attraction of the whole earth is composed of a countless number of forces. The resultant of the combined attraction of all the particles of the earth is directed towards the center of the earth.

If we take into consideration the enormous mass of the earth in comparison with one gram of mass, we can easily understand the very small value of the constant of gravitation compared to one gram weight, because the gravitation constant is numerically equal to the force which 1 gram exerts on 1 gram, and 1 gram weight is nearly equal to the force which the whole earth exerts upon 1 gram of mass. But the earth is rotating and thus produces a centrifugal force.

Centrifugal force is everywhere perpendicular to the axis of rotation, or parallel to the earth's equator, and its magnitude is proportional to the mass of the bodies (m), to the square of the angular velocity of the earth (w), and to the radius of rotation (q), hence: $C = mqw^2$.

The greatest value of the centrifugal force is at the equator (3.39 dynes per gram of mass), diminishing towards the poles, where it disappears entirely.

The Niveau Surface of Gravity.—It is now evident that gravity has at every point one special value and that its direction and magnitude change with the location. Such a force would be represented by so-called *lines of force,* or by *niveau surfaces.*

We will suppose that a particle moves through space in such a way that the direction of the movement corresponds to the direction of the force everywhere. The path described by this moving particle is in general a curved line. This line is known as the *line of force,* because the tangent to a point on this line is the same as the direction of the force in the same place.

If such a system of the lines of force is constructed in space, the direction of the force at every point in space becomes known. Further, we can always construct surfaces that are perpendicular at every point to these lines of force. These surfaces may be termed the *niveau surfaces* of the forces.

As the free level of a liquid at rest is always perpendicular to the forces acting upon it, the level of the calm sea is a niveau surface of gravity. We shall consider the niveau surface which coincides with the level of the sea as the shape of the earth, the so-called geoid. The special function of gravity will be known if the shape and dimensions of the geoid and the values of gravity at every point are determined.

Observations in many parts of the earth have shown that the geoid surface cannot correspond to a simple geometrical figure, but there is a possibility of finding a geometrical figure which not exactly, but approximately, corresponds to the shape of the earth. It is an ellipsoid of rotation, of which the axes, according to Bessel's measurements, are as follows:

semi-minor axis (axis of rotation) = 635,607,895 cm.
semi-major axis (axis of equator) = 637,739,716 cm.

Bessel's ellipsoid may be regarded as the ideal figure in representing the earth.

Likewise the changes of gravity cannot be expressed by means of any analytical formula with complete accuracy, but a formula can be found which approximately corresponds to the real values of gravity. This is Helmert's formula: $g = 978.00\ (1 + 0.00531\ \sin^2 \phi)$, where ϕ indicates the geographical latitude.

In this formula, gravity has the same magnitude at every point of the same circle of latitude, but is different in the different circles of latitude; it is smallest at the equator and increases toward the poles, where it is largest. The values which are calculated by means of Helmert's formula are called the normal values of gravity.

Bessel's ellipsoid represents the earth's form and Helmert's formula expresses the values of gravity upon it; they do not correspond to the true shape and values, but are only approximate. An orange is approximately spherical, but on its outer surface are many small eminences and depressions, and in judging the shape of the orange we take into consideration not only its spherical form, but also its small irregularities. Likewise the niveau surface of gravity, the so-called geoid, is only approximately an ellipsoid of rotation, with its elevations and depressions, which correspond to the shape of the earth. These elevations and depressions are caused by visible and subterranean mass disturbances. This may be illustrated by an example.

Assuming that we have determined gravity on a limited part of an immensely extended plane, and that the density of the subterranean masses is the same everywhere (for instance 1.8), we shall find that gravity has the same magnitude and direction at every point; the arrows representing gravity have the same length and are parallel to each other. The niveau surface of gravity is a horizontal plane (*i.e.*, in Fig. 86 the line N–N would be horizontal and the arrows vertical and of equal size).

Now we will suppose that there is underground a mass of greater density (for instance 2.6). As a result of the attraction of this mass, gravity at different points is not of the same power, and the values are not parallel as before; the attraction increases towards the underground mass and its direction inclines towards

it. This result is illustrated in Fig. 86. The actual change in magnitude and direction of gravity is not, however, as great as is shown by the figure. The changes are as small in proportion as the gravitation constant—too small for representation.

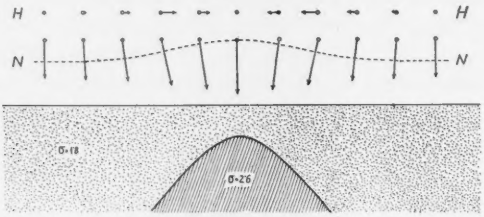

FIG. 86

The niveau surface is now no longer a horizontal plane, but is elevated above the denser subterranean mass (N–N in Fig. 86). Mountains, valleys, and other geographical forms also influence the shape of the niveau surface. It is therefore evident that the niveau surface cannot be considered as a regular geometrical figure; we can only say that it is a varied curved surface.

THE SPACIAL DISTRIBUTION OF GRAVITY.

Determination of the Deviation of the Niveau Surface from the Spherical; the Horizontal Directing Force of Gravity.—We can form an idea of the appearance of the niveau surface if we know its deviation from the spherical form at every point. For this purpose we must first determine the curved portions of the niveau surface.

P represents any point on the niveau surface. The surface near this point is in general not of a regular geometrical shape. The question arises as to how we may determine the relationships of the curvature of the surface at this point.

If we make a perpendicular section through the point P, it will be a curved line from the surface. The radius of the circle which coincides most closely with the curve at the point P is called the radius of curvature of this cross-section. But we can

make an infinite number of perpendicular cross-sections at the point P, each with a different radius of curvature. Among these there is always one which is the smallest, r_1, and one which is the largest, r_2. There two radii are called the *principal radii of curvature* and the two sections, the *principal sections.* The principal cross-sections are always at right angles to each other.

The reciprocal values of the principal radii of curvature, $1/r_1$ and $1/r_2$, are called the principal curvatures of the surface at the point P, which express the curvature of the surface. If their value is great, then the surface is much curved, but if they are small, it is less so. The difference of the principal curvatures multiplied by the gravity $R = [(1/r_1)-(1/r_2)]\ g$ is what Eötvös called the horizontal directing force of gravity. In this expression, since r_1 is the smaller, R is always a positive quantity.

If the form is a perfect sphere, $R = 0$ $(r_1 = r_2)$. The greater the deviation from the spherical form, the greater R becomes; therefore R gives an idea of the rate of the deviation of the niveau surface from the spherical form.

According to Eötvös, the horizontal directing force can be represented graphically. For this purpose we shall draw on the horizontal tangent plane of the niveau surface a line through the point P, which will be in the direction of the principal section of the smallest curvature, and its length proportional to the magnitude of the horizontal directing force. If we indicate in this manner the values of R at different points, we acquire a graphic representation of the curvatures of the niveau surface.

Components of Gravity Tangent to the Niveau Surface.—As the niveau surface is not in general a horizontal plane, and gravity is directed always at right angles to the niveau surface, gravity in general has components in the horizontal plane. Let H–H in Fig. 87 represent the horizontal plane belonging to the point P in the niveau surface N–N; it then becomes evident from the figure that the horizontal component of gravity vanishes only at the point P, whereas at all other points it differs from zero. Consequently there exist in this horizontal plane different forces which have resulted from the fact that the niveau surface is not a plane,

but a curved surface, since the directions of gravity are not parallel to each other at the different points in the niveau surface.

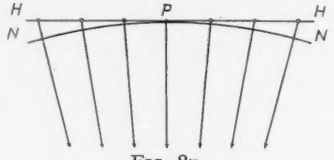

FIG. 87

Fig. 88 represents the horizontal components of gravity, as well as gravity and its horizontal components in the two principal sections of the niveau surface as a whole. The niveau surface shows the least curve in the direction indicated by *I–I*, and the greatest in *II–II*. The horizontal components of gravity incline towards the least curved section *I–I*. Consequently if we suspend a movable beam (*a–b* in Fig. 88), which can turn freely in the

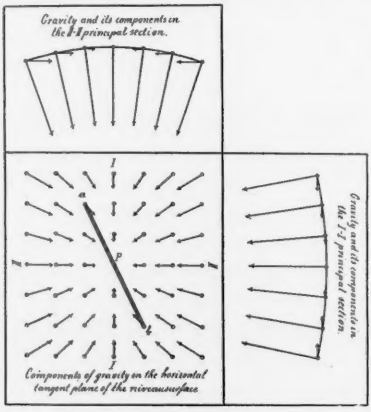

FIG. 88

horizontal plane, these components of gravity will tend to turn the beam towards the less curved section. Only the spherical form of the niveau surface is an exception to this rule, as all the

horizontal components of gravity are directed towards the point *P*, so that they have no rotatory power. This rotatory power, which acts upon the suspended beam, depends upon the value of the horizontal components of gravity, and this depends upon the amount of deviation of the niveau surface from the spherical form. The value of this deviation is represented in *R*. It can be proved that this rotatory power is proportional to the value of *R*. For this reason Eötvös called the value of *R* the horizontal directing force of gravity.

Thus the first part of the problem is solved; the value of *R* is determined by the deviation of the niveau surface from the spherical shape.

Gradient of Gravity.—If we take any point on the niveau surface and go outward from it in any direction in the horizontal plane, we shall find one direction in which the value of gravity increases the most. The amount of increase of gravity in this direction for a distance of 1 cm. characterizes the change of gravity on the niveau surface. Eötvös called this quantity the *gradient of gravity*.

According to Eötvös the gradient may be represented by arrows, drawn upon the horizontal plane in such a way that their direction coincides with the direction of the greatest increase in the value of gravity, and their length is proportional to the value of the gradient.

We have indicated the gradients as mentioned in the case above on the horizontal line *H–H* in Fig. 86. At a great distance from the subterranean mass the gradients vanish; they become greater nearer the mass, and again vanish when they are directly above the mass. As the gradients are directed towards the subterranean mass, they give a visible representation of the changes in gravity on the niveau surface.

The quantities by which we may characterize gravity on the niveau surface are the magnitude of the horizontal directing force and its direction, and the magnitude and the direction of the gradient. To determine these quantities at different points of the earth's surface is difficult, as they are so extremely small that they

form but a few billionths of the force of gravity as a whole. Eötvös, however, has constructed such an extraordinarily sensitive instrument that it is possible to determine the magnitude and direction of the gradients and of the horizontal directing force of gravity.

EXPERIMENTAL METHOD.

For this purpose Eötvös has constructed the so-called "Variometer of curvature," and the "Horizontal variometer." The first may be used to determine the magnitude and direction of the horizontal directing force, and the latter the magnitude and direction of the gradient.

Variometer of Curvature.—We have seen that on account of the deviation of the niveau surface from the spherical form there are components of force in the horizontal plane which tend to turn the suspended beam in the direction of the smallest principal curvature of the niveau surface (see Fig. 88). Eötvös used this principle to determine the horizontal directing force.

FIG. 89

The variometer of curvature is a Coulomb torsion balance (as in upper part of Fig. 89). It consists of a light beam suspended upon a very thin platinum wire, and on the two extremities there are platinum weights. Since the horizontal components of gravity tend to turn the beam towards the smallest principal curvature

of the niveau surface, the wire will be twisted. The momentum of rotation depends on the magnitude and direction of the horizontal directing force of gravity. This torsion produces an elastic force in the wire, which tends to turn the beam back. The result of these two momenta of rotation equilibrates the beam. If we know the angle of torsion and the momentum of torsion of the wire, we can establish the momentum of rotation of gravity.

We define the position of the beam with reference to the angle which it makes with the north-south direction. We call this angle measured from north to east the *azimuth* of the instrument. If we set the instrument in different azimuths and observe the torsion angles of the beam in each case, we may calculate the magnitude and direction of the horizontal directing force.

Horizontal Variometer.—In this instrument (Fig. 89) one of the platinum weights is suspended from one of its extremities. Through this change the determination of both the magnitude and the direction of the gradient of gravity becomes possible.

If the gradients are equal to zero in every point of a limited part of the earth (if *N–N* in Fig. 86 were horizontal and arrows vertical and equal), the forces of gravity are parallel at different heights, since the lines of force are vertical. But if the gradients

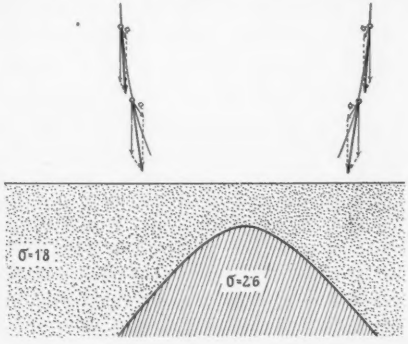

FIG. 90

differ from zero (as in Fig. 86), the forces of gravitation are not

parallel at different heights because the attraction a (Fig. 90) of the subterranean dome of greater density grows downwards and inclines to the subterranean mass. The lines of force of gravitation are not straight, but curve downwards in the direction of the gradient; therefore the forces of gravitation are not parallel at different heights (see arrows of Fig. 90). Consequently the two forces affecting the two platinum weights of the horizontal variometer (Fig. 89) have different directions; they are not parallel with the axis of rotation, but incline toward it at different angles.

Let O stand for the point of suspension (Fig. 91) and S for the

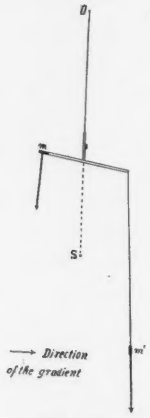

FIG. 91

center of gravity of the balance. The wire coincides with the line OS. This line indicates the direction of gravity at point S and is also the axis of rotation. If we suppose that the gradient is directed from left to right, the force acting upon the upper mass m inclines towards the left of the axis, while upon the lower mass m', towards the right. Therefore these two forces have components in the plane of rotation (the plane which is at right angles to the axis). These components tend to adjust the beam parallel to the gradient, and according to the theoretical considerations, they are proportional to the gradient.

Therefore the moment of rotation of gravity acting on the balance may be considered as composed of two parts: one due to the deviation of the niveau surface from the spherical form, and the other to the curvature of the lines of force. The value of the first moment depends upon the horizontal directing force, and the latter upon the gradient. Through these two moments the balance will be turned, and thereby produce a third moment, which is the torsion moment of the wire. This moment equilibrates the balance with the other two moments. This condition can be expressed in one equation, which contains four quantities to be determined; namely, the magnitude and direction of gravity, and the magnitude and direction of the horizontal directing force. To these four unknown quantities is added a fifth—the unturned position of the balance. It is necessary to have five equations for the determination of these five unknown quantities, which may be obtained by placing the instrument in five different azimuths and determining in each case the equilibrial position of the beam, which is to be measured by a scale fixed to the chest of the torsion balance. In this way we obtain five equations from which the unknown quantities may be calculated.

The instrument itself is very simple, composed of a torsion balance on which the two masses are suspended at different heights. It must be well protected from any outside influences, especially radiations; hence it is enclosed in a triple-walled torsion chest. Fig. 92 shows the instrument. The chest with the enclosed balance can be turned around the axis of the entire instrument and adjusted in any azimuth on a horizontally divided circle.

The most essential part of the instrument is the wire, since by its turning the measurements are made. It is made of platinum-iridium of a thickness of 0.04 mm. and approximately 56 cm. in length. First of all the wire must be fixed, or the balance will not have a permanent position. The determination of the position of equilibrium is made from telescopical readings. For this purpose there is a small mirror fixed upon the balance, and on the chest of the instrument a telescope with a scale. The time of oscillation of the balance is very great on account of its great

sensibility. Its simple time of oscillation is approximately 15 minutes.

The gradient and the horizontal directing force can be determined by means of Eötvös's instrument with an accuracy of 1.10^{-9} C.G.S. units.

FIG. 92

The formulæ obtained, by means of which the gradient and the horizontal directing force of gravity can be calculated, are determined in the laboratory. The proceedings of the measurements are as follows:

With the aid of the compass the instrument is set so that the suspended weight points directly to the North (I position of the instrument). Then the instrument is left alone until the balance has come to rest. This requires about an hour with the new instruments. The next step is to read with the telescope the position of the balance on the scale.

After having read the position of the balance in the I position, we adjust it successively in the II, III, IV, and V positions, and proceed as before. With these five results the unknown quantities can be calculated by the aid of the formula of the instrument.

This operation requires much time. Eötvös shortened it by constructing a double variometer (Fig. 93). It consists of two

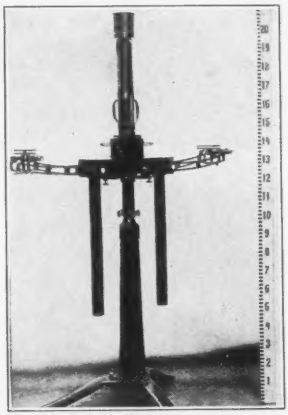

FIG. 93

simple balances, placed so that they are turned 180° from each other. With this double variometer it is only necessary to establish the position of the balance in three directions—the north-south direction, and the directions subtended by the same and angles 120° and 240°.

Recently observations have been made in the open air with the double variometer exclusively. These observations were made in a tent with padded walls, from 9 P.M. to 6 A.M., so as to eliminate any possible disturbances from radiations of the sun.

DETERMINATION OF SUBTERRANEAN DISTURBANCES.

The stations of observation are from three to five km. apart, according to the changes of gravity of the locality. In localities where the changes of gravity are sudden, more frequent observations are made.

The gradients and the horizontal directing forces are the values which are produced by the entire mass of the earth with both its

visible and subterranean irregularities. If we wish to know the existence of gravity and its niveau surface on the surface of the earth or on some part of it, we must take into account the topography of the earth. Our maps, however, do not show all the small irregularities, although their effect on the instrument might be very great in the immediate vicinity. Hence it is necessary to take into consideration the effects of the immediate surroundings. For this purpose the ground must be levelled on the plane of observation in a circle with a radius of 3 meters; the angles of inclination to the horizontal plane must be determined in the north-south and east-west directions, and then the height of the immediate surroundings must be read with a level in 8 directions at distances of 5, 20, 50, and 100 meters. The effect of the irregularities of the immediate surroundings upon the instrument can be calculated by the aid of these results and the density of the uppermost layer of the earth. Eötvös called these values *terrain corrections.*

If we subtract the terrain corrections from the (observed) real values, we obtain what corresponds to the topographical features shown by the map. We call these values the *topographic values;* thus topographic value = real value — terrain correction.

As the instrument is set by the aid of a compass, the values have to be changed from magnetic to true north.

The topographic values are produced by three effects—the normal earth, the visible, and the subterranean irregularities. Hence if we subtract from the topographic values the effects of the normal earth and of the visible irregularities, there only remains the value produced by the subterranean irregularities. Knowledge of this makes it possible to draw conclusions regarding the distribution of the underground masses.

On the normal earth (Bessel's ellipsoid) the gradients and the horizontal directing forces have certain values. According to Helmert's formula, gravity has the same magnitude in every point of the same circle of latitude, but it is different in the different latitudes, increasing gradually towards the poles. Therefore the gradients are parallel everywhere to the meridian,

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and their direction in the northern hemisphere is towards the north, and in the southern hemisphere towards the south. Their values vanish at the equator, grow towards the poles, and have their greatest value (8.17×10^{-8} C.G.S.) at 45° latitude; then again they diminish towards the poles, where they vanish. These are the so-called *normal values of the gradient.*

Bessel's ellipsoid is understood to be the normal shape of the earth. As this form is not a sphere, but an ellipsoid of rotation, the values of the horizontal directing force differ from zero. The direction is everywhere at right angles to the meridian; their values are at their maximum at the equator, and diminish towards the poles, where they vanish. They are the so-called *normal values of the horizontal directing force.*

If we subtract the normal values from the topographic values, we get the values which come from the topographic deviations from the normal shape of the earth. We call these differences topographic disturbances.

Topographic disturbance = topographical value — normal value. The topographical disturbance is the result of two effects, the visible and the subterranean irregularities.

The effects of the visible irregularities (mountains, valleys, etc.) can be calculated from contour maps. These are *cartographic effects.* The difference between the topographic disturbance and the cartographic effect is produced by the subterranean irregularities. For this reason Eötvös called the difference the *subterranean disturbance.* Subterranean disturbance = topographic disturbance — cartographic effect.

In this way we may calculate the magnitudes and directions of the gradients, and then of the horizontal directing forces produced by the subterranean irregularities, with reference to every station of observation. From these values we can draw conclusions as to the deviation of gravity and its niveau surface, caused entirely by the subterranean irregularities.

Let G be the value of the gradient at a point. This means that the increase of gravity in the direction of the gradient for a distance of 1 cm. is equal to G. But if we progress s cm. in the

direction of the gradient, then the increase of gravity is *Gs*, and if the direction of the progression does not coincide with the direction of the gradient, then the change of gravity is equal to the projection of *Gs* upon the direction of the gradient. If the absolute value of gravity is known in one point, and the values of the gradients are determined in places close enough to each other, we can then calculate with the aid of the above-mentioned method the values of gravity in every point of the observed territory.

If we connect with lines all the points which have the same values of gravity, we obtain a map of the distribution of gravity. These lines are called *isogam lines.*

Fig. 94 represents observations near Arad. The heavy line

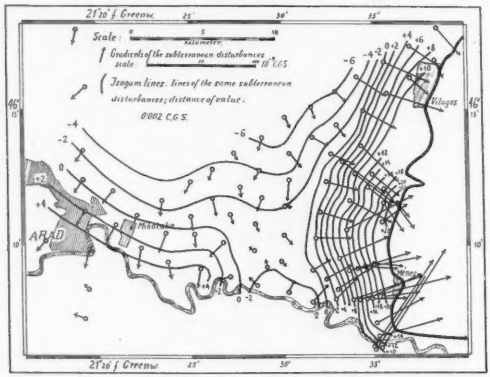

FIG. 94

indicates the boundary between the mountainous and level territories; the arrows represent the gradients, and the connecting lines the isogam lines of the subterranean disturbances. It is noticeable that the gradients are directed towards the mountain territory, which indicates that the rocky mass of the mountain extends underground under the level country towards Arad. The numbers appearing in the figure are the values of the subterranean disturbances of gravity in 1/1000 C.G.S. unit.

Fig. 95 illustrates our observations in the vicinity of Kecskemét, with the gradients and isogam lines which indicate the subterranean disturbance. We can assert, therefore, that gravity has its minimum effect west of Kecskemét, where the subter-

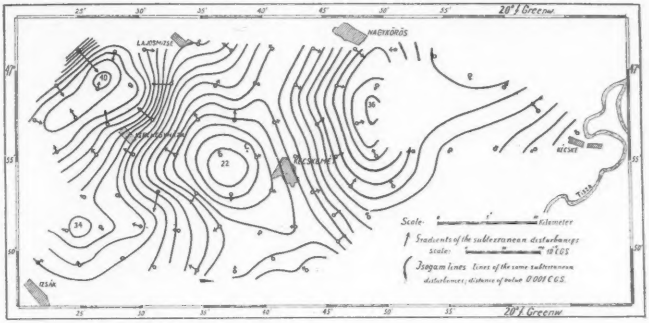
Scale:
Kilometer
Gradients of the subterranean disturbances
Isogam lines lines of the same subterranean disturbances; distance of value 0.001 C.G.S.

FIG. 95

ranean disturbance of gravity has the value of 0.022 C.G.S. The gradients are directed outward from this minimum, which means that the greater underground masses lie outwards. The maximum of gravity is toward the northwest with a value of 0.040 C.G.S. On the opposite side also the gra-

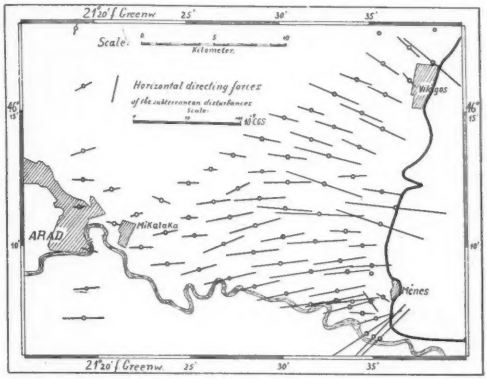

FIG. 96

dients come towards it, which indicates that a large mass exists under the point 40. The existing values are better reproduced by the isogam lines. It is interesting to know that the epicenter of the great earthquake of July 17, 1911, was in the neighborhood of the minimum of gravity, represented by the letter *C* in the figure.

Fig. 96 shows the values of the horizontal directing forces in the vicinity of Arad. The straight lines represent the horizontal directing forces. From the illustration we may infer that the niveau surface is curved the most in the north-south direction, while in the east-west it is less so.

PRACTICAL APPLICATION.

The precise accuracy with which the torsion balance determines the change of gravity makes the determination of the distribution of underground masses possible, and can be used to great advantage.

Fig. 94 shows that the rocky masses of the mountain extend beneath the level surface of the region, and enables the depth of the underground masses to be determined by simple calculations. Suppose the density of the denser mass to be 2.6 and that of the upper mass 1.8, then the diminishing of gravity by 0.001 C.G.S. means that the denser mass is 30 meters deeper.

Therefore the isogam lines in Fig. 94, where the changes of gravity between two neighboring isogam lines are equivalent to 0.002 C.G.S., correspond to contour lines indicating a variation of 60 meters. Figs. 97 and 98 represent the cross-cut of this territory through Ménes, in geographical latitude 46°8′ and in the meridian of 21°35′ east of Greenwich. Similarly on the

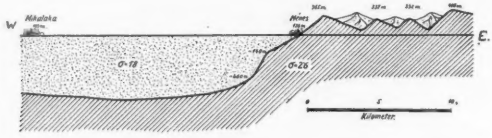

FIG. 97

map of Kecskemét (Fig. 95) the isogam lines succeed each other with differences of 0.001 C.G.S., and therefore correspond to contour lines of 30. meters. The section passing through the 22d minimum and the 34th and 36th maxima of gravity shows that there is a crater-like widening of the denser mass beneath the plain, upon the side of which the city of Kecskemét was built.

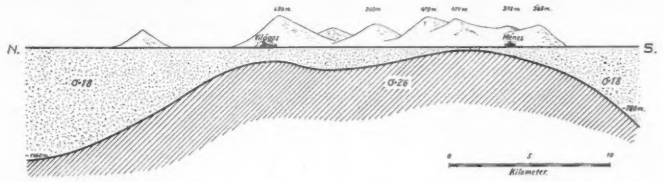

FIG. 98

Two other examples illustrate the practical importance of this method.

Suppose that there is underground a bed of greater density as is illustrated in Fig. 99. Theoretical calculations show that in

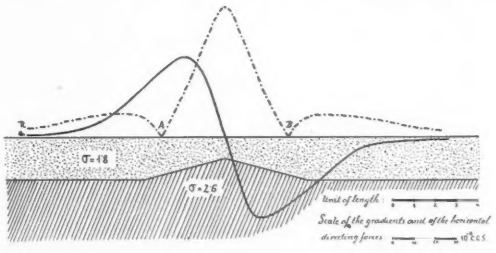

FIG. 99

this case the gradients become greater as we approach nearer to the bed and attain a certain maximum; then they vanish as they pass over the bed, further on have negative values, attaining a certain minimum, and then gradually become greater again. These changes of the gradient are illustrated in Fig. 99 by the solid line. The values of the horizontal directing force (R,

dotted line) also vary, attaining a certain absolute maximum just above the bed. The horizontal directing forces are parallel with the bed everywhere on the plain between *A*–*B* (Fig. 99), but are perpendicular to it on the plain outside the *A*–*B* territory.

For the second example, suppose that the denser stratum beneath the plain has an elevation like a step in a staircase (Fig. 100). In this case the values of the gradients have everywhere

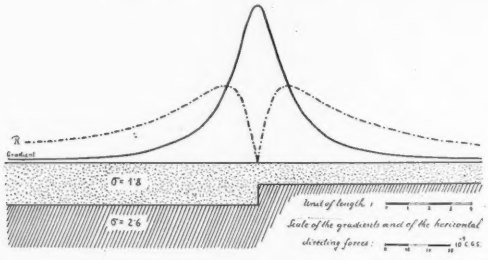

FIG. 100

positive values. Their directions are everywhere the same, directed to the step from left to right; the maximum value is just above the step. The values of the horizontal directing force have two maxima, and one minimum just above the step. The directions of *R* in the territory to the left of the step are everywhere perpendicular to the stratum, and in the territory above the step, parallel to it.

In these two examples the distribution of the mass beneath the plain was indicated, and the values of the gradient and of the horizontal directing force were calculated. Conversely, if the distribution of the mass beneath the plain is unknown, and we determine the gradients and the horizontal directing force by Eötvös's experimental method, obtaining the same changes of value as in Fig. 99, we may conclude that the underground mass at this point is similar to that in Fig. 99; or perhaps the values may correspond to those illustrated in the case of Fig. 100, when

a step formation may be inferred. If the difference in density of the two masses is known, the depth of the underground stratum may also be calculated.

It is thought that this method will prove successful in mineral explorations. Although we cannot definitely determine the distribution of underground masses by its aid, we may, however, draw important conclusions, if we are familiar with the general geological formation of the surrounding territory. While it is possible for geologists to determine the structure of mountainous regions, it is often difficult to determine the underground strata in level country. Eötvös's gravity method, however, can be used with great success in level regions, giving an idea of the formation of anticlines and domes, of the folding of strata, and of the boundary lines of underground salt masses and mineral deposits. It is therefore of practical value to economic geologists.

The Eötvös balance has been employed in Hungary since 1903 at first purely for scientific investigations, and later for mineral investigations. Recently it has been used by d'Arcy Exploration Co. (Anglo-Persian Oil Co.) and some associated Companies of the Royal Dutch Co. for petroleum and gas exploration. It has also been used in Germany, Italy, Croatia, Austria, Egypt, and Mexico.

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UNIVERSITY OF BUDAPEST,
HUNGARY.

AN ARIZONA ASBESTOS DEPOSIT.[1]

ALAN M. BATEMAN.

INTRODUCTION.

Asbestos enclosed in limestone and formed as a result of alteration by igneous intrusion is sufficiently unusual to attract attention. One thinks customarily of asbestos associated with serpentine that is derived from the alteration of basic igneous rocks by circulating heated waters, for the important asbestos deposits of the world so occur. But Noble and Diller have shown that the asbestos deposits of the Grand Canyon region of Arizona were formed by the action of diabase on magnesian limestone. Later, a similar origin was attributed by Diller to other Arizona asbestos deposits. One of these deposits, at Sierra Ancha, was examined by Mr. J. F. McClelland and myself, and the following notes deal with the observations and studies made at that time. The general occurrence of these deposits has already been mentioned briefly by Diller and Sampson,[2] so that these notes add but little new material to their observations; the purpose is to present more detailed knowledge of this peculiar asbestos deposit and a discussion of its origin, which, it is hoped, may throw light on the origin of asbestos veins in general. For example, it will be shown that the Sierra Ancha asbestos veins were not formed by pushing apart their walls.

GENERAL FEATURES.

The field examination included the Sierra Ancha deposits and the Wolf-Hardeman group, some twenty-five miles distant from Sierra Ancha, but the following notes are confined entirely to the Sierra Ancha mine. This property is owned by the American

[1] Presented before the Society of Economic Geologists, New York Meeting, May, 1923.

[2] U. S. Geol. Surv. Min. Res. Part II., for 1917 to 1921.

Ores and Asbestos Company, and is in the Sierra Ancha Mountains of Arizona, just north of Roosevelt Lake, at the head of Pocket Creek. It is about 30 miles northeast of Globe, Arizona.

The discovery of asbestos in the Grand Canyon region stimulated prospecting in other parts of Arizona with the result that in 1913 silky fiber was discovered on Ash Creek, near Globe. Extensive development followed; the property came under the control of the Johns Manville Co. and since then there has been a small but continuous production of asbestos.

Another discovery of asbestos was made on the Wolf-Hardeman group near Young, in 1914, and still later, excellent silky fiber was found at the head of Pocket Creek in the Sierra Ancha Mountains. The American Ores and Asbestos Co. was organized to work these deposits and commenced shipments in 1917. This company then came under control of the U. S. Asbestos Co. of Lancaster, Pa., and the Raybestos Co. of Bridgeport, Conn., and a more vigorous development program was started; mining equipment was installed, about a mile of underground workings was run, and shipments of spinning fiber were made. Following our examination, Mr. E. B. Shutts was placed in charge of the property with instructions to confine attention to extraction of crude spinning fiber only, without regard to mill fiber, and to exploration for more long fiber.

Subsequently, several other asbestos properties were exploited in the Globe district,[3] but to date the American Asbestos Association and the American Ores and Asbestos Co. have been the largest shippers.

TOPOGRAPHIC SETTING.

The southern end of the Sierra Ancha range is almost detached from the main mountain mass, so that it forms a nearly isolated butte. On its undulating wide top, at an elevation of about 6,300 feet, is the camp of the American Ores and Asbestos Co. To the south and east the mountains descends 2,000 to 3,000 feet, with steep and in places precipitous slopes, to a gently in-

[3] See "Asbestos in 1920," by Edward Sampson, U. S. Geol. Surv. Mineral Resources, 1920, Part II., pp. 309–316.

clined plateau below. This plateau in turn falls off by long ridges and intervening canyons to a rolling, gently sloping plain which extends to Roosevelt Lake (elevation 2,123 feet) and the Salt River. On the northern slope, the butte descends abruptly into Pocket Creek. The mine workings lie just under the rim of the butte on the precipitous south-facing slope.

GEOLOGIC SETTING.

The topography of the butte, with its flattish top and steep slopes, affords excellent exposures of the rocks on three sides. A section of the rocks at the mine is as follows:

Rock.	Thickness in Feet.	Description.
Sandstone	30–50	Red gray and black. Altered and hardened at base.
Upper limestone	3	Altered impure limestone, trace of asbestos.
Upper diabase	2–12	Diabase sill, irregular in thickness.
Middle limestone	20–35	Altered gray magnesian limestone; contains asbestos.
Middle diabase	480	Diabase sill.
Lower limestone	130	Top is altered to a hard reddish rock.

The downward continuation of the section has been shown by Diller.[4]

The rocks of most importance from the standpoint of the asbestos deposits are the diabase and the middle limestone—the diabase because its intrusion was the cause of the asbestos formation, and the middle limestone because it is the host rock of the commercial deposits.

Structure.—The rocks lie nearly horizontal—a feature that imparts to the mountain its flattish top and steep slopes. On the south side of the mountain, they dip gently to the north; on the north side, some 3,000 feet across, they dip gradually southward, and the mine workings show that the contacts are slightly undulating. Thus the outcrop of any one rock may be traced on the steep slopes around three sides of the mountain. (See Fig. 101.)

[4] Diller, J. S., U. S. Geol. Surv. Min. Res., 1917, 1919, Part II., pp. 199 and 301.

The diabase intrusion is in the form of a huge sill which follows closely the bedding planes of the sedimentary rocks. Here and there it cuts across the beds at a gentle angle, but its upper contact was not found to cross more than two 20-foot topographic contours. An apparent transgression of the diabase sill across the sedimentary formations on the southwestern side of

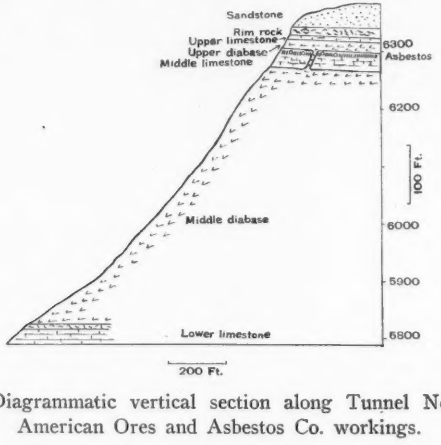

FIG. 101. Diagrammatic vertical section along Tunnel No. 5 of the American Ores and Asbestos Co. workings.

the mountain, amounting to 140 feet, proved to be due to a reverse fault that elevated the southwestern corner of the mountain by this amount. The highest knob on top of the mountain marks the site of this upthrown block.

The upper, middle, and lower limestone are unquestionably each a part of the same formation—believed by Diller [5] (following Ransome [6]) to be Mescal limestone. It will be noted from the geologic section given above that they are separated by the upper and middle dibase, both of which are a part of the same intrusion. (Fig. 101.)

The diabase intrusion in general followed the contact between

[5] *Loc. cit.*, 1919, p. 300.

[6] Ransome, F. L., "The Copper Deposits of Ray and Miami, Arizona," U. S. Geol. Surv. Prof. Paper 115, 1919.

the Mescal limestone and the overlying sandstone, but in the vicinity of the asbestos occurrence, a lens-like sliver of limestone three feet thick retained its original position to form the upper limestone. At the same locality a larger lens of limestone, 20 to 35 feet thick and 1,650 feet in length was broken off from the main body and is completely surrounded by diabase. A smaller lens of limestone occupies the same position on the west side of the hill and another large one, 1,900 feet long and 17 feet thick, is on the north side. It is in the upper parts of these included lenses that the productive asbestos occurs and the one on the south side of the hill contains much more fiber than the one on the north.

At one place a crack occurred in the limestone inclusion and the diabase magma flowed through this so as to connect the sills above and below, and upon solidification formed a dike. (Fig. 101.) Adjacent to this dike the fiber is more abundant and of better grade.

Diabase.—The normal diabase is greenish black, medium grained, and even textured, but near its contacts the grain becomes notably dense within a space of 6 to 8 inches. Under the microscope the diabase is seen to be made up of basic feldspar and augite, with lesser amounts of olivine and brown mica, and a little magnetite. The pronounced decomposition of the augite, olivine, and feldspar, shows that heated solutions permeated the diabase after it had crystallized.

Sandstone.—The sandstone for a few feet above the diabase sill has suffered intense alteration as a result of the intrusion; it has been baked and silicified. Part of it has taken a whitish-gray or dull reddish color. Its resistance to erosion causes it to stand out as a strong cliff, locally termed the "rim-rock." A short distance above the altered sandstone, normal flaggy sandstone is the rule.

Limestone.—The upper, middle, and lower limestones are each of somewhat different composition and in consequence have been differently affected by the diabase intrusion. The upper and lower limestones contain considerable impurities in the form

of clayey-matter and chert, and on weathering present a gnarled appearance. The top of the lower limestone, in contact with the diabase sill, has been baked for a depth of 3 feet to a dense hard reddish rock. Below this for another few feet it has been altered to a dirty gray-colored crystalline limestone, which gradually passes into the dark gray normal impure limestone. The upper limestone has been crystallized to dolomite marble, and in immediate contact with the upper diabase it has been silicified.

The middle limestone is a magnesian limestone of gray color and is relatively free from impurities. This difference in purity apparently has been an important factor in the localization of the asbestos in the middle limestone to the exclusion of the others. Upon alteration it has yielded serpentine, whereas the upper and lower limestones were of such composition that minerals other than asbestos or serpentine tended to form under the conditions of alteration that gave rise to the asbestos deposit. Hence, in this locality the upper and lower limestone can be excluded as containers of commercial quantities of asbestos.

The middle limestone is most altered in its upped part, just beneath the upper diabase, and where the alteration is most intense the most asbestos occurs. The alteration consists in the formation of vari-colored narrow bands, parallel to the bedding and therefore nearly horizontal. Layers of light gray, finely banded, crystalline dolomitic lime, alternate with light and dark green bands of a dense smooth substance which resembles serpentine, and with bands of dense, waxy-looking, light-colored material which in appearance and fracture somewhat resembles opal. The asbestos is usually associated with the latter. The waxy substance is apparently retinalite. The bands of alteration vary in width from two inches to two feet, and are usually separated by bands of partially recrystallized limestone.

THE MINERAL DEPOSIT.

Character of Asbestos.—The Sierra Ancha asbestos, as well as the other Arizona fiber, possesses rather distinctive qualities. Its most important characteristic is its negligible quantity of

magnetite. This distinction forms an important contrast to the Canadian asbestos and gives to the Arizona fiber its chief value for use in the industries. The lack of magnetite makes it an excellent insulator for electrical purposes, particularly for the making of asbestos insulated wire for uses in which a pliable wire with thin insulation and the highest non-conductivity are essential. For such purposes it excels the Canadian fiber. It was the importance of this feature that led to our detailed examination of the property.

Two varieties of asbestos occur—the silky and harsh. The silky variety is an unusually soft and pure chrysolite with excellent spinning qualities. It varies in color from white to buff shades, and when spun yields a pure white yarn. All sizes of the silky variety occur; some No. 1 Crude has been found up to 6 inches in length, and 4-inch fiber without partings is not uncommon. Partings in the fiber are less frequent than in most asbestos occurrences.

The harsh variety is so named because it is splintery and somewhat brittle. Small bundles of fiber will prick the fingers and crack upon bending. There are all grades of harsh fiber, from that which is only slightly splintery to a variety so harsh that individual fibers cannot be separated. The extremely harsh material cannot be utilized; the slightly harsh grade can be spun but yields a splintery yarn; the intermediate material, which constitutes the greatest amount of the harsh asbestos, can be used for spinning fiber only after a preliminary acid wash. The silky and harsh varieties are scattered indiscriminately through the deposit; they may occur in separate veins close to each other or in the same vein.

The largest amount of harsh fiber contained in the shipments to the United States Abestos Co. caused them to have several analyses made in an endeavor to find the cause of the harshness. The following analyses are quoted from Mr. Diller,[1] No. 1 being soft fiber and No. 2 harsh fiber:

[1] Diller, J. S., U .S. Geol. Surv. Mineral Resources, 1919, Part II., p. 302.

	No. 1.	No. 2.
MgO	40.69	41.41
SiO_2	40.75	42.28
Al_2O_3	1.82	1.07
FeO	.74	.88
CaO	none	.10
H_2O	1.86	1.33
H_2O+	12.65	12.23
	98.51	99.30

The analyst, Dr. Zimmerman, points out [8] the differences in alumina and the fact that CaO occurs only in the harsh fiber, but suggests that the small amount of calcite could hardly account for the brittleness of the harsh fiber. Mr. Diller points out [9] that the harsh fiber usually has a deposit of a thin film of calcite between the fibers. Mr. Diller further states that his attention was drawn to veins of fibrous calcite apparently pseudomorphous after chrysotile and that Mr. Sampson suggests that the fibrous calcite may be parallel growth instead of a replacement of chrysotile.

Some chrysotile may have been replaced by calcite, for outcrop specimens were observed with fibrous structure and regular asbestos partings, and which have the appearance of being pseudomorphic after chrysotile, but they are whitish in color and effervesced vigorously with acid. Other specimens under the microscope showed veinlets of fibrous calcite, but there was nothing to indicate whether it is simply a fibrous growth of calcite, or replacement of chrysotile. Still other specimens of harsh fiber effervesce slightly in acid, but are made up of chrysotile. On the other hand, much of the purest of the harsh fiber gives no effervescence with acid and under the microscope is seen to consist wholly of chrysotile with no visible calcite. Therefore, all the harsh fiber cannot be attributed to replacement by calcite.

Examination so far has not shown conclusively the cause of this unusual variety of chrysotile. Chemically and optically it

[8] *Idem.*
[9] *Idem.*

cannot be distinguished from the silky variety. I am inclined to believe that it represents either another variety of chrysotile or an incompleted stage of transformation of massive serpentine to chrysotile, or that the harshness may be due to a later infiltration of serpentine (retinalite) around the chrysotile fibers.

The general character of the chrysotile may also be gathered from the production by sizes. The ratio is:

1 ton crude (No. 1 of 1½ in. +, No. 2, ⅝ to 1¼ in.)
to 2.14 tons of harsh long fiber,
to 3.42 tons of "mill fiber" (¼ to ⅝ inches),
to 46.3 tons of mill rock (which can be utilized only by milling).

The mill ore yields about 18 per cent. of its weight in commercial grades of fiber.

Occurrence.—The asbestos occurs as horizontal seams of cross fiber chrysotile in bands of serpentine in the middle limestone, and parallel to its bedding. Only the upper part of the middle limestone, near the upper diabase, has proved productive; the lower two thirds of it has been tested in a few places with disappointing results. One to three bands of serpentine may occur in the upper part of this limestone, one or more of which may contain asbestos. In some areas the serpentine may be present without asbestos. One band of serpentine, notably persistent throughout all of the workings, lies within about two feet of the upper diabase, and in most places contains some asbestos. Practically all of the serpentine lies within a six-foot zone of the limestone. The individual serpentine bands vary from a few inches to 24 inches in width, and from one to fifteen individual seams of asbestos of varying sizes may occur within a serpentine band; usually there are three to five.

The serpentine is mostly a brownish-green variety with waxy lustre and conchoidal fracture, but some is deep green, black, brown, or yellowish. The individual bands may themselves be distinctly banded due to color differences of the different varieties. In several places the central part of the serpentine bands

consists of an unusual whitish, waxy-looking substance which resembles opal and appears isotropic. Tests made upon it by Professor W. E. Ford show that it is made up of silicate readily decomposable in acid, much magnesium, much water, a trace of aluminum, no calcium, and has a hardness of 4-5 and fusibility of 4-5. Presumably it belongs to the serpentine group. Determination of its index of refraction in oils gives 1.53±. Its properties do not coincide with any of the minerals listed in Larsen's Tables,[10] but in composition and index of refraction it nearly approaches that of spadaite. Its physical properties, however, agree with the variety of serpentine called retinalite, and it was recognized as such by Dr. E. T. Wherry.[11] It is most abundant immediately adjacent to chrysotile, and accompanies the harsh more often than the silky fiber.

The asbestos always occurs in the bands of serpentine and is localized in shoots that are separated by blank or poorly mineralized areas. Within the shoots, the fiber is spotty; rich areas pass abruptly to lean areas, long fiber to short fiber, and harsh to soft, and vice versa. Mill rock, mill fiber, and long harsh are rather intimately intermingled, but the soft crude occurs only in scattered pockets.

It was determined by means of careful measurements made at five-foot horizontal intervals throughout all of the workings, that the average thickness of all the asbestos veins in the main zone is 0.654 feet, and this contains an average recoverable content of spinning fiber of 14 per cent. It was further calculated that each 1,000 square feet of mineralized area in the main ore zone would yield:

0.1 tons No. 1 crude
0.29 tons long harsh
0.34 tons mill fiber
17.5 tons mill rock

Localization.—As has already been pointed out, the asbestos is in serpentine bands, and the latter occur in the upper part of

[10] Larsen, Esper S., "Microscopic Determination of the Non-opaque Minerals," U. S. Geol. Surv. Bull. 679, 1921.

[11] In informal conversation.

the middle limestone. The upper diabase has been the important factor in localizing the asbestos-bearing serpentine, for all of it lies within eight feet of the overlying diabase contact. The diabase dikes which cut the middle limestone are also important localizers of asbestos; the best fiber mined lay close to the diabase dike in No. 5 Tunnel. Also the thin edges of the middle limestone lens, where diabase is close both above and below, is particularly favorable for the development of pure serpentine and chrysotile.

The composition of the limestone was also important in localizing the chrysotile; the best fiber occurs in the magnesian limestone, where it is free from impurities, whereas the impure beds of the limestone are barren. Thus the impure upper and lower limestone are uncongenial for asbestos.

The promiscuous intermingling of harsh and soft, and short and long fiber, appearently follows no rule of localization.

Distribution.—The asbestos is known over a length of 1,650 feet and it has been followed into the hill for a distance of 325 feet from the south side of the mountain. On the north side the host limestones extends for a total distance of about 1,900 feet, but is for the most part unexposed. The superior quality of the asbestos on the south side, as contrasted with the north side, indicates that a given amount of exploratory work would meet with more promising results in the south. The north and south workings are 3,000 feet apart and it is conjectural if asbestos is distributed between them; the lens-like character of the containing limestone suggests that the deposit will not continue uninterruptedly between them. Over the greater part of the mountain the middle limestone is absent and only that area underlain by the middle limestone will afford any likelihood of yielding asbestos upon exploration.

Some 556,000 square feet of area were delimited in which further work would probably yield crude asbestos, and an additional 1,500,000 square feet were outlined which might be asbestos-bearing.

ORIGIN OF THE ASBESTOS.

Among the many ideas that have been advanced for the origin and peculiar arrangement of chrysotile in veins, two general explanations have attained prominence: One, by Taber,[12] that the chrysotile veins made room for themselves, not by replacement of serpentine, but by pushing apart their enclosing walls, and that they owe their structure to the fact that the material for growth was supplied only at the base of the growing crystals through supersaturated solutions occupying small openings; and the other by Dresser and Graham,[13] that the chrysotile veins grew, not by pushing apart their walls, but "at the expense of the walls," and that the fibrous growth took place owing to the tendency of the fissures to open, due to rock contraction by cooling, and the chrysotile fibers were able to grow only in the direction of least pressure.

A study of the origin of the unique Arizona occurrences throws light on these two explanations.

The asbestos deposits in the Grand Canyon have been shown by Diller[14] and Noble[15] to have been formed as a result of a diabase intrusion. Diller has also pointed out[16] the similarity between the Grand Canyon deposit and those near Globe.

The writer's investigation of the Sierra Ancha deposits corroborates the conclusion of Diller. The asbestos is formed only where there is serpentine, and the latter occurs only near the diabase; the formation of the serpentine and asbestos must have been caused by the intrusion of the diabase sills. They were, however, not always formed together, for bands of serpentine occur without chrysotile. A small amount of contact metamor-

[12] Taber, Stephen, "Genesis of Asbestos and Asbestiform Minerals," *Trans. Am. Inst. Min. Engrs.*, vol. 57, 1917, pp. 62–87; 95–98. ECON. GEOL., vol. 12, 1917, pp. 476–479.

[13] Dresser, J. A., Geol. Surv. Can. Memoir 22, 1913. Graham, R. P. D., ECON. GEOL., vol. 12, 1917, pp. 154–202; *Trans. Amer. Inst. Min. Engrs.*, vol. 57, 1917, pp. 91–93.

[14] Diller, J. S., U. S. Geol. Surv. Min. Res., 1907, Pt. II., pp. 720–721; *idem*, 1908, p. 705.

[15] Noble, L. F., U. S. Geol. Surv. Bull. 549, 1914, pp. 59–60.

[16] U. S. Geol. Surv. Min. Res., 1917, p. 201.

phism, shown most abundantly in the overlying shaly limestone, accompanied the diabase intrusion but it is not known definitely if the serpentinization of the limestone was produced by direct contact metamorphism or by later hydrothermal metamorphism due to waters given off from the cooling magma. Under any circumstances it would be difficult to draw a line between the two. Noble states that the asbestos is "a product of the contact metamorphism of the limestone by the diabase, and, as Diller suggests, the serpentine that encloses the veins of asbestos is probably derived from some mineral in the limestone and not from the diabase."[17] Noble states further[18] that the serpentine "shows no trace of alteration from pyroxene, hornblende, or olivine."

The writer is inclined to the belief that the formation of the serpentine was due to the later hot water emissions from the magma and that most of the silica and probably much of the magnesia were contributed by the diabase, rather than a straight recrystallization into serpentine of ingredients already present in the limestone. Some magnesia was originally present in the limestone, but the amount was small; a little silica was also present. These materials, however, were not present in sufficient abundance to yield serpentine by simple recombination. Where impurities in the limestone are abundant, minerals other than serpentine were formed, and the serpentine occurs only along beds of relatively pure limestone.

That silica did come from the diabase is indicated by the intense, though small, silicification of the sedimentary rocks adjacent to parts of the diabase contacts. The limestone next to the serpentine was altered to a finely crystalline marble and much calcite occurs as thin bands and grains within the serpentine itself. The bands of serpentine occur so definitely along certain of the limestone strata that the composition of the beds must have been a controlling factor in the formation of the serpentine. Serpentine is absent along cross-fractures and joints in the limestone.

[17] *Loc. cit.*, p. 59.
[18] *Idem.*

The solutions that brought about the formation of serpentine and chrysotile are considered to have been a part of the same igneous activity that gave rise to the diabase and that they reached the sedimentary rocks after the diabase had crystallized. The pronounced hydrothermal alteration that the diabase has undergone shows that heated solutions traversed it. The resulting decomposition of the olivine and feldspars in the diabase may alone have supplied all the magnesia and silica necessary for the formation of serpentine and chrysotile in the limestone.

As mentioned previously, much of the serpentine is the variety retinalite, and thin sections show that this is clearly replacing calcite. All gradations of replacement may be seen, from grains of calcite with only the margins altered to serpentine, through grains in which the alteration is so advanced that the calcite twinning and grain outlines are just visible, and finallly to massive serpentine with its confused fibrous structure that produces the effect of low interference colors or even appears to be isotropic; the outlines of the original calcite grains may still be seen faintly. In places, residuals of unreplaced calcite remain in the massive serpentine. The replacement starts either at the boundaries of interlocking calcite grains or along minute cracks parallel to the bedding. Where the limestone is but incompletely converted to serpentine there may be bands of serpentine alternating with fresh or slightly changed limestone.

The development of chrysotile seems in general to be slightly later than the serpentinization, and a study of thin sections shows that the chrysotile has grown by the replacement of serpentine, and to a lesser extent, of carbonate. In addition to the larger veins, numerous little veinlets of chrysotile are revealed in the areas of serpentine. They may occur as solid bands or ramifying interlacing veinlets with parallel orientation. The larger bands contain unreplaced residuals of serpentine or calcite. Many smaller veinlets can be seen clearly to follow minute cracks that cross both grains of serpentine and calcite. In the calcite the veinlets are constricted, but widen out in the serpentine, often replacing the whole serpentine grain.

The margins of the chrysotile veinlets have a frayed and jagged appearance where the chrysotile extends into the serpentine. Delicate feathery edges of slender chrysotile fibers project, without bending, ahead of the main chrysotile veinlet into the serpentine, in a manner that precludes any other process except replacement. Where a mosaic of serpentine and carbonate grains occurs, the serpentine grains may be wholly or partially altered to chrysotile, giving a mosaic of chrysotile and carbonate grains.

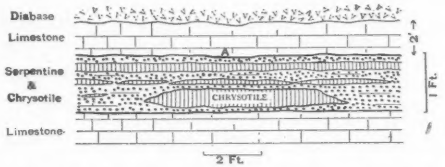

FIG. 102. Sketch of chrysotile occurrence in Tunnel No. 5, to show that the horizontal bedding in limestone and serpentine is undisturbed opposite 6½ inches of chrysotile (Point *A*), indicating impossibility of growth of chrysotile by pushing apart its walls.

In a few cases, the carbonate is also replaced directly by chrysotile without the intervening formation of serpentine. Where this occurs, the orientation of the chrysotile is irregular. Cases were observed of single serpentine areas with chrysotile growing from both sides with parallel orientation, where a few bundles of more rapidly growing fibers met in the center, leaving unchanged serpentine on either side. Such features can hardly be interpreted in any other manner than by replacement. Indeed, all the thin sections indicate in many ways that the chrysotile has been formed by replacement of serpentine.

There is nothing to indicate, either in the larger veins or in the microscopic veinlets, that the chrysotile has grown by pushing apart its walls, as has been so vigorously contended by Taber.[19] On the other hand, there is absolutely conclusive evidence afforded by this unusual type of deposit that no pushing aside of the walls took place. The best instance of this was seen in Tunnel No. 5, where an eleven-inch band of serpentine was exposed in

[19] Taber, Stephen, "Genesis of Asbestos and Asbestiform Minerals," *T. A. I. M. E.*, vol. 57, 1917, pp. 62–87.

the limestone at a distance of two feet beneath the horizontal roof of diabase. Both above and below the serpentine band there were well-marked horizontal bedding-planes in the limestone, and the serpentine, as usual, displayed prominent color banding parallel to the bedding. A persistent 1½ inch asbestos vein was enclosed in the serpentine as in Fig. 102. At one place beneath it, also in the serpentine, there was a one-inch, and a short four-inch chrysotile vein, indicated at point *A*, Fig. 102, making a total of 6½ inches of chrysotile at this point. The bedding-planes above and below the serpentine were parallel and horizontal at the point *A*. Obviously, if 6½ inches of chrysotile in an eleven-inch serpentine band had grown by pushing aside its walls, the bedding planes would have shown a bulge at this place. Therefore, this occurrence could have originated only by the growth of chrysotile at the expense of the material that previously occupied its place.

It is, therefore, clearly evident that all asbestos veins have not been formed as the result of growing chrysotile pushing aside its walls and thereby making room for itself mechanically. In the case of the Sierra Ancha deposits, at least, it is rather, as expressed by Graham[20] for the Canadian asbestos veins, "that this growth has taken place at the expense of the walls, which . . . have receded because they were continually destroyed, and not because they were pushed apart by growing fibers."

The writer does not, however, find himself in agreement with Mr. Graham, in so far as the Arizona deposits are concerned, in the idea that the growth of chrysotile is dependent upon the tendency for the fissures to open, thereby producing less pressure normal to the walls and enabling the fibers to grow in this direction. Although a cooling intrusive, such as the Quebec peridotite, might possibly be called upon to produce the necessary contraction simultaneous with chrysotile growth, certainly no such forces can be considered where asbestos is in limestone. Taber has already shown[21] that crystal fibers may be elongated

[20] Graham, R. P. D., *Trans. Amer. Inst. Min. Engrs.*, vol. 57, 1917, p. 92.

[21] Taber, Stephen, "Pressure Phenomena Accompanying the Growth of Crystals," *Proc. Nat. Acad. Sci.*, vol. 3, 1917, pp. 299–300.

in the direction of greatest pressure, provided they are in contact with supersaturated solutions only in that direction.

The chrysotile of the Sierra Ancha deposit must be considered to have grown, not by pushing aside its walls, nor by crystallization in the direction of least pressure resulting from contracting walls, but by actual replacement of the serpentine composing its walls, through the agency of hydrothermal solutions. The peculiar structure of the chrysotile may have been caused, as is stated by Taber,[22] by the solutions supplying the material for growth at only the base of the crystals. That crystals may grow in this manner has of course been proved experimentally in the laboratory, but it is difficult to conceive of such nice adjustments in one of nature's asbestos deposits by which solutions, particularly in such a relatively porous rock as limestone, could be supplied at only one end. General experience relating to alteration and replacement of minerals in rocks and ore deposits indicates that waters usually have access to more than one side of a growing mineral. Possibly there may be some other reason, as yet unknown, that determines this peculiar crystallographic habit of chrysotile. Whatever the reason, it is clear that chrysotile does grow in the direction of the elongation of its fibers. The force of crystallization occasioned by such growth—a force that Taber considers to have been great enough to push apart the vein walls —may instead be considered by reason of the pressure exerted on the adjacent serpentine to have brought about more ready solution of the massive serpentine and thereby give room for the growing chrysotile.

Where a growing mineral is in contact with one that it is replacing, the pressure resulting from the growth of the replacing mineral should aid or bring about the solution of the mineral being replaced. This idea, to my knowledge, has never been proved in the laboratory, but if it could be established experimentally it would supply an understanding of how chrysotile replaces serpentine, and indeed would elucidate the whole problem of replacement.

[22] *Idem.*

In the Sierra Ancha deposit where replacement of serpentine by chrysotile is demonstrated, it is not clear what started the initial transformation of serpentine to chrysotile. Probably it is to be ascribed to slightly changing solutions emitted from the diabase, under the influence of which the massive serpentine became unstable and chrysotile the stable form. The similarity in composition between serpentine and chrysotile has been advanced as an argument against the transformation from one form to another by instability of one. But though the chemical composition of the two may be almost identical, it is evident that there are different serpentines of diverse optical and physical properties. The variations in hardness, fusibility, and indices of refraction prove this. Different varieties of chrysotile are also shown by the harsh and silky varieties of Arizona and the divergence of recorded refractive indices.

The horizontal position of the cross-fiber veins parallel to the bedding is also difficult to understand under the hypothesis of replacement. Two factors may have influenced this: one is, that the travel of the solutions was along the bedding planes, as is shown by the several parallel bands of serpentine separated from each other by horizontal layers of unaltered limestone; the other is that the direction of greatest pressure was vertical, parallel to the orientation of the chrysotile fibers, and normal to the bedding.

The hypothesis outlined above for the origin of the Arizona chrysotile incorporates the results of the experimental work of Taber and others as to the force of growing crystals, and the ideas of Graham and Dresser that the chrysotile was formed "at the expense of the receding serpentine walls," and also satisfies the conditions of occurrence for the Arizona asbestos. Possibly it may apply to other asbestos deposits.

It gives me pleasure to acknowledge the benefit received from discussions and criticisms on the part of my colleagues Professor Adolph Knopf and Professor William E. Ford, of the Department of Geology.

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DISCUSSION.

Mr. Loughlin: Mr. Diller showed me a few years ago some of this Arizona asbestos in which he thought that the harsh fiber was due to little films of calcite sandwiched in among the fibers of asbestos itself, and a suggestion was raised whether such fiber could be milled and perhaps in the presence of dilute acid the calcite eliminated and the harsh fiber converted into flexible fiber. I should like to know whether Dr. Bateman can throw us any further light on that question.

Mr. Bateman: Mr. Chairman, the harsh fiber is a splintery fiber that I did not take time to describe in the short time at my disposal; sometimes it is so splintery that individual particles of it will break between the fingers. There are all degrees of harshness, from that which is so harsh that it can not be bent or separated to that which is just slightly harsh. It has been found and utilized by the United States Asbestos Company that by giving it a preliminary acid wash the fiber may be softened and spun. The silky fiber makes an unusually fine spinning fiber. The harsh fiber made a very prickly thread and was not at all suitable for spinning; but by this preliminary wash it could be spun. As to what is the cause of the harshness, I do not know. Naturally, I was interested in Mr. Diller's suggestion of its being due to infiltration of calcite, but in a considerable amount that I tested I could find no evidence of calcite; in some of the very harshest there was not a trace of calcite visible by acid test, or under the microscope. I thought that possibly the harshness might have been due to a slight infiltration of the retinalite, a variety of serpentine, around the fibers, and that it could not be distinguished under the microscope because its optical properties are almost the same as those of chrysotile itself.

Mr. Bastin: The whole question of the origin of these veins which show fibrous characteristics is a very interesting one, and I suspect that the solution does not lie along a single line. Certainly, Professor Bateman's suggestion that some of them are formed by replacement—perhaps the majority are—aids us greatly in understanding how a great mass of serpentine such as that of Thetford, Quebec, for example, could be penetrated by a ramifying mass of fibrous veinlets. It is difficult to conceive of the expansion that would result from the growth of crystals taking place among so many veinlets without its manifesting itself in structural deformations of bordering formations that would be quite obvious.

I think it is worth while however to call attention to one fibrous veinlet that observed a few weeks ago that appears to have involved the pushing aside of two sides of a fracture. The specimen was an unlabeled one in the University of Chicago collections. It shows dark gray talc traversed by a veinlet of fibrous calcite as shown in Fig. 103. The vein-

let resembles in structure many of the asbestos veinlets developed in serpentine. Its walls are of knife-edge sharpness; it possesses a "parting" wandering somewhat erratically through the median portion of the vein; the calcite fibers are essentially perpendicular to the general plane of the vein but are not perpendicular to minor irregularities in its walls.

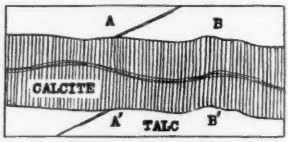

FIG. 103

A minute veinlet also of fibrous calcite occupies the position shown at A and A′. Calcite fibers in the larger veinlet trend from the end of the small veinlet at A to the end of the other veinlet at A′. The small calcite veinlets do not continue across the larger veinlet.

These relations suggest, though they do not demonstrate that the walls of the large veinlet once fitted together, A and A′ forming a continuous fracture. The fact that concavities in one wall are matched by convexities in the other as at B and B′ also supports this interpretation as does finally the sharpness of the walls. Had the larger veinlet been developed by replacement, beginning at a fracture now marked by the median "parting," the ends of the smaller veinlets should not be directly opposed to each other nor should the walls of the larger vein be sharp and their irregularities matched. Whether the walls were parted prior to the calcite deposition or by the force of crystal growth of the calcite in questionable but the latter hypothesis is at least reasonable in view of the experimental work of Becker and Day.[1]

Mr. Chance: It seems to me that Dr. Bastin's figure presupposes that the calcite veinlet was formed before the main vein which is supposed to have caused the dislocation. Possibly this assumption is not supported by facts. Further, it seems to me that a fracture of this kind could be produced without disturbing a vein—a fracture could be produced which would subsequently become filled with calcite or any other material by a force acting at an angle to, rather than the direction of the plane of the section (Fig. 103). Such a force might readily fracture the material without fracturing the vein, if the vein was more resistant, because to develop a fracture requires only an infinitesimal actual movement. And then the fracture having developed, any force, gravitation, or otherwise,

[1] Geo. F. Becker and A. L. Day, "Note on the Linear Force of Growing Crystals," *Journal of Geology*, vol. XXIV. (1916), pp. 313–333.

might separate the walls of the fracture sufficiently to permit a vein of any size to be deposited. The fact of the two ends of the fracture in this case being exactly opposite might be purely accidental.

Mr. Spurr: It seems to me, without having any knowledge whatever of the subject, that this diagram of Mr. Bastin's simply shows that one part has been split off from the other. In other words, a fissure exists here, a filled fissure; but I do not see that the diagram of itself indicates what the splitting force was, which has formed that fissure; and we may, it seems, with equal probability assume any of several different types of forces.

A. C. Spencer: I will refer to Dr. Bastin's drawing and to his suggestion that the walls of the chrysolite vein were pushed apart by the force of crystallization. Clearly the rock masses above and below the horizontal vein have been separated, but to my mind the conclusion does not necessarily follow that the vein has made its own space. Another way out would be to assume that the vein grew in slack space provided through the operation of external forces. A slight crevice opened by differential movement would permit initial deposition. Then, as further movement occurred, and as space became available, growth would continue so long as the required solutions were supplied.

My suggestion is that some veins may have grown by permission rather than by insistence, and I am inclined in this direction for an explanation of some of the big veins which are precedent and Mr. Graton would regard as injected masses somewhat analogous to igneous dikes. I suspect that the case that Dr. Bastin has illustrated here might be resolved without the conclusion that the force of crystallization has pushed the walls apart and lifted great masses of material.

DEEP-SEATED OXIDATION AND SECONDARY ENRICHMENT AT THE KEELEY SILVER MINE.

J. MACKINTOSH BELL.

INTRODUCTION.

In 1912, when the writer paid a brief visit to Canada, he was led to recommend to his London principals the acquisition of the Keeley mine, near Cobalt, Ontario, on account of certain broad structural principles apparent on the surface. Previous to this date, the property after a bright beginning had had an unsavoury career and had been closed since 1911. Operations were conducted during 1913 and in the following year till the outbreak of the war stopped promising endeavours which were not actively renewed until 1920.

As the work progressed, it was found necessary to modify considerably the original conclusions on account of structural peculiarities which were not obvious on the surface, and even more so because of the discovery of highly oxidized sections in several parts of the property. One of these oxidized sections at least, is not shallow, but has been followed downward level by level and is still conspicuous at the deepest horizon yet attained, namely, 560 feet.

What is the reason for this relatively deep oxidation—so unusual in a region which has been generally so intensely glaciated? What effect has the oxidation had upon the segregation of values? The desire to answer these questions forms the excuse for this paper.

TOPOGRAPHY.

The Keeley mine is situated in the district of South Lorrain, about sixteen miles southeast of Cobalt, of which it forms a geological outlier, and to which it is subsidiary from a commercial

standpoint. The mine lies on the crest of hills nearly 600 feet above and about three miles west of Lake Temiskaming. The rise from Lake Temiskaming is not steady, the intervening country being broken by several ridges separated by relatively deep valleys containing lakes and streams. These valleys have commonly a general parallelism to the main axis of the adjacent portion of Lake Temiskaming.

Southwestward of the Keeley is the deep basin of Trout Lake, with its main axis again lying north and south or approximately that of Lake Temiskaming. Roughly, the shape of Trout Lake is that of an isosceles triangle, with a width of about half a mile at its northern end and narrowing to a few yards wide at its southern extremity, one and one half miles to the south, where a small stream issues draining the lake down a north and south depression to the Montreal River. Both the eastern and western shores are high and abrupt, but from its northeastern bay a depression in the hills radiates northeastward, and another northward, through and beyond the Keeley property. From the northwestern bay, a third but less marked depression passes northwesterly. These various topographical features require emphasis in view of the fact that certain of them have apparently a marked bearing on the problems presented.

GENERAL GEOLOGY.

Though the depressions of the 80-acre tract covered by the Keeley mine are relatively deeply filled with boulder clay and much of the rest of the property shrouded in similar glacial débris, exposures of solid rock are of common occurrence. These consist of Keewatin rocks, lamprophyre dikes, and Nipissing diabase, which have been so fully described elsewhere[1] that an elaborate petrographical description need not be given.

The Keewatin series in the vicinity of the mines of South Lorrain is represented by a greenstone which is ordinarily of such fine texture and so much metamorphosed that its original character is generally difficult of diagnosis. A petrographical range

[1] A. G. Burrows, "South Lorrain Silver Area," Nineteenth Rep., Ont. Bureau of Mines, Part II., 1913, p. 134 et seq.

from acid andesite to basalt is apparently represented, in the form of lavas and volcanic sediments.

The lamprophyre dikes which ramify in all directions through the greenstones, are in proximity to the veins difficult to distinguish from the Nipissing diabase in a similar position, but generally the greater shearing and the presence of large plates of an altered mica serve to differentiate the two.

Dikes of an acidic rock cut the Keewatin in places, but are rarely found near the veins. While they are thought to represent residual acidic segregation from the Nipissing diabase[2] their actual genetic connection with the latter in South Lorrain has not yet been indicated.

The diabase of South Lorrain is petrologically identical with that of Cobalt. It is commonly sufficiently coarse to exhibit microscopically the characteristic ophitic structure. The presence of quartz shows it to be essentially a quartz-diabase.

The diabase occurring as a wide sill has been intruded in such manner as to assume the form of a broad dome, which has been truncated by erosion. The veins of the Keeley are associated with the westerly dipping limb of the dome and occur either in the diabase itself or in the Keewatin rocks beneath which the sill plunges. The dip of this westerly limb varies from 10° to 40°, but the increasing depth below the surface, as one passes westerly, is not constant within the variations of these angles, owing to the fact that the structure is complicated by minor rolls in the sill and by numerous premineralization faults transverse to the strike of the diabase-Keewatin contact (approximately north and south), and by several others, of more pronounced character, generally parallel thereto.

The faulting in the vicinity of the Keeley has a marked influence on the topography as previously indicated, and the comprehension of the latter, in suitable geological surroundings forms an important guide to prospecting. While all of the principal faulting expressed at the mine seem to have occurred mainly, at least, prior to the mineralization, it must not be supposed that con-

[2] W. G. Miller, Nineteenth Rep. Ont. Bureau of Mines Part II., 1913, p. 104.

versely all the faults are mineralized. The faults studied in the Keeley workings appear to be reverse in character; the greatest throw not exceeding fifty feet. While the faulting in South Lorrain has been, as yet, only very broadly studied, the surmise that all the minor dislocations are connected with the major movement, primarily responsible for the deep basin of Lake Temiskaming,[3] forms an attractive and probable hypothesis, for which there is considerable supporting field evidence.

CHARACTER OF THE VEINS.

What may be described as the "Mother Lode" of the western section of South Lorrain is Wood's vein, which has been traced in the underground workings almost across the Keeley and located also in the properties to the north and south. Along its generally north and south strike, it has a length of at least a mile. The pronounced fault which it occupies resembles the Cobalt Lake fault, in being the fracture of paramount control in influencing the distribution of the veins, but it differs therefrom in the remarkable strength of the vein-material contained and in the smaller amount of gouge. With a general parallelision to Woods occur in the Keeley a number of other north and south veins, and roughly transverse to it, a multiplicity of others.

The Keeley veins so closely resemble those of Cobalt in the association and paragenesis of the minerals contained; in the origin of their ore-shoots; in their structural characteristic; and in other details, that it will be advisable here to emphasize the differences rather than to reiterate features which have already received much attention from various writers.[4]

[3] See M. E. Wilson, Memoir 103, Geol. Surv. Can., p. 34 et seq.

[4] W. G. Miller, Nineteenth Rep. Ont. Bureau of Mines, Part II., pp. 5 et seq.

Edson S. Bastin, "Significant Mineralogical Relations in Silver Ores of Cobalt, Ontario," ECON. GEOL., vol. XII., p. 219 et seq.

W. L. Whitehead, "The Veins of Cobalt, Ontario," ECON. GEOL., vol. XV., p. 103.

Alfred R. Whitman, "Genesis of the Ores of the Cobalt District of Ontario," Univ. Cal. Pub., vol. 13, No. 7, pp. 253–310.

Cyril P. Knight, "Cobalt—Its Past and Future," *Eng. and Min. Jour.-Press*, May 6, 1922, p. 762 et seq.

The Keeley veins like those of Cobalt are ordinarily narrow, varying in width from a few inches to a few feet. Wood's vein and certain others do not always occupy a single fracture but commonly a fractured zone, which may be from five feet to (in the case of Wood's) even ten feet in width. Within this fractured zone, vein material proper does not ordinarily extend from wall to wall, but occurs in several more or less pronounced stringers. While the intervening altered country rock may be more or less impregnated with silver values, it is frequently—even adjoining very high-grade shoots—almost barren, and in contrast to the veins in the Cobalt series at Cobalt, there is ordinarily little or no impregnation of silver values beyond the generally pronounced walls.

While the deposits at Cobalt occur in "narrow, practically vertical fissures and joint-like cracks," [5] those of the Keeley occupy faults along which there was pronounced dislocation [6] prior to the deposition of the vein-material. While in the minor veins there seems to be a tendency for the segregation of the values where the dip is vertical or approaching verticality, Wood's vein, which is the most important silver producer of the Keeley, has an average dip (easterly) of only 60°, and rich shoots occur where the inclination is even much flatter.

While the richest ore-shoots of the Keeley occur close to, but not exactly at the contact between the diabase and greenstone, and important ore bodies are found alike in both rocks, the vertical range over which ore extends from the contact seems to be greater than in the case of Cobalt. Thus, rich ore occurs in Wood's vein to the lowest horizon of the mine, 480 feet (where it is wholly in diabase) or a length along the dip of not less than 550 feet, and how much greater remains to be seen.

The unaltered vein material is composed of country rock more or less replaced by carbonates (chiefly calcite but with some dolomite), quartz, and metallic minerals. The metallic minerals

J. Mackintosh Bell. "The Occurrence of Silver Ores in South Lorrain, Ontario, Canada," *Bull. Inst. Min. and Metal.*, Feb., 1922.

[5] W. G. Miller, *loc. cit.*, p. 5.

[6] See Alfred R. Whitman, *loc. cit.*, p. 281.

show the same variety as at Cobalt. Smaltite, cobaltite, chloanthite, niccolite, pyrite, marcasite, arsenopyrite, chalcopyrite, native silver, argentite, ruby silver in various forms, and native bismuth are all common. Galena and sphalerite are rarer, but are apparently more conspicuous than at Cobalt. The quantity of iron-bearing sulphides contained seems to be generally much higher than at Cobalt, and it is possible that the percentage of iron in the smaltite is greater than is ordinarily the case at Cobalt.

Significant as may be the various differences between the character of the Cobalt veins and those of the Keeley, already enumerated, much more impressive is the degree to which alteration of the original vein material has taken place at the Keeley as compared with Cobalt. There has been great alteration by meteoric waters, in many veins in various parts of the property, to depths of 100 feet or more, while the change is so intense in the southern 500 or 600 feet of Wood's vein, that it will be advisable to explain it in detail.

The first level to be opened on the portion of the vein in question was at a depth of 230 feet. Here the vein-material consists mainly of a soft puggy clay. Where this is completely oxidized (iron-stained—brown, yellow, and red; rarely copper-stained—green and blue; and still more rarely, showing traces of Cobalt bloom) the quantity of silver contained, except uncommonly in the form of thin scales,[6a] is negligible even in locations structurally suitable for its deposition. On the other hand, in the partially oxidized portions, that is, where partly in the "ferrous" rather than completely in the "ferric" state, the amount of silver present is considerable. In other words, there are shoots of mill ore with irregularly distributed values. The oxidation at 230-foot level extends into the adjoining walls, especially the foot wall, and is equally conspicuous along No. 20 vein, which branches southeasterly from the hanging wall.

At the next level down—300 feet—conditions are similar, though the quantity of silver contained in the "ferrous" vein-material is less important. At 360 feet the vein-material is

[6a] C. W. Knight. The South Lorrain Silver Area, Bull. 48, Dept. of Mines, Ont., p. 28.

almost wholly oxidized; there is little or no bluish pug characteristic of the "ferrous" phase; the vein fissures being filled mainly with firm brown and yellow clay, almost devoid of silver.

At 420 feet, much "ferrous" material occurs, associated with the more wide-spread "ferric," with the consequent great enhancement in silver. The demarcation between "ferrous" and "ferric" is frequently extraordinarily abrupt, but it is still noteworthy that purely "ferric" material, even a few inches away from rich "ferrous" ore is practically valueless. The silver in the "ferrous" ore occurs in the wire form as argentite, in spongy accumulations of tiny scaly crystals resembling miniature stalactites, and as thin plates. Complete analyses were made by Messrs. Ledoux & Co., from the 420-foot level of two samples taken from a few feet of each other. The samples were entirely representative and taken right across the vein. No. 1 is what is described as "ferrous" ore, and No. 2 "ferric" vein material.

	No. 1.			No. 2.
	Pulp, %.	Metallics, %.	Composite, %.	%.
S_2O_3	43.24		39.6	45.46
Fe	10.33	4.69	9.9	10.67 (Fe_2O 15.24%)
Al_3O	12.06		11.1	19.92
CaO	2.42		2.2	1.69
MgO	6.92		6.3	3.92
MnO	0.68		0.6	1.33
As	10.36	10.42	10.4	0.38
Sb	0.53		0.5	Trace
Cu	0.07		0.1	0.06
Co	0.33		0.3	0.28
Ag	0.33		0.3	
Ag	4.59	73.36	10.3*	0.01 (= 3 oz. per ton)
S	0.26		0.2	0.10
P_2O	0.30		0.3	0.20
Au	Trace	0.06		10.47
H_2O	—	—	—	Trace.

* Silver 3008.5 oz. per ton of 2,000 lbs.
Gold 0.88 oz. per ton of 2,000 lbs.

In No. 1 it will be noticed that the elements determined in the pulp account for only 92 per cent. The balance is made up of oxygen combined with some of the iron and with other elements; combined water as an essential part of the gangue; and the alkali

metals—potash and soda. The calculation of the composite is on the assumption that the metallics do not contain the constituents of the gangue matter, but, of course, some gangue constituents are included in the metallics. The metallics consist essentially of native silver, silver arsenides, and iron.

In sample No. 2 no metallics were contained. The iron was unquestionably present, chiefly as ferric oxide (Fe_2O_3) but an exact determination of the state of oxidation was impossible on account of the presence of arsenic, sulphur, and perhaps other oxidizable elements.

The presence of so large a quantity of silver arsenide in the metallics of the first sample is interesting and even more so the relatively large quantity of gold. Samples of the ordinary rich primary ore—not associated with the altered zones—have been tested on various occasions for gold, but with negative results.

At the 480-ft. level, the present deepest horizon of the mine "ferrous" vein-material predominates over "ferric" resulting in longer and more persistent ore-shoots. At this horizon much more smaltite is apparent on Wood's vein than at the levels above (especially in what seems to be only slightly altered material), but by no means in the quantity or of the same character as found in the entirely unaltered ore in other parts of the mine. In No. 20 vein, branching from the hanging wall, the vein-material is almost wholly "ferrous" and the silver unassociated with cobalt minerals, in the wire form, or in the peculiar "stalactites" previously described at the level above.

No. 26 vein joining the foot-wall of Wood's vein to that of No. 28 which branches at right angles from Wood's vein, consists partly of altered vein material and partly of unaltered; the progressive phases of alteration having been visible as the drift on it was opened up. The vein dips vertically, joining Wood's and No. 28 upward on their respective dips near the 420-ft level. The vein in its length of 80 feet excepting in the few places where it is highly "ferric," consists of exceedingly rich ore, at one point being nearly five feet wide of 7,000 ounce material. At this widest point more than half of the vein consists of normal

silver-bearing smaltite, with a little calcite generally similar to unaltered ore in other parts of the mine. In the remainder, oxidation has begun, some of the smaltite has been attacked, and cobalt bloom, and a little iron rust are noticeable, coating the silver in the cellular spaces.

CAUSE OF THE DEEP OXIDATION.

The suggestion has been made[7] that the great depth of oxidation along the southern part of Wood's vein and the lesser extent in other parts of the property may be due to climatic changes since the close of the glacial period. This hypothesis may be correct, but the writer is not inclined to view it with favor, on account of the general absence of even superficial oxidation in the district in general, and because of the fact that the oxidized areas of the Keeley are invariably beneath relatively deep depressions filled with boulder clay. It is believed the deeply oxidized sections of the veins represent rather the remnants of a generally departed zone[8] which was mainly eroded during the glacial period.

The relatively much greater depth to which oxidation extends in the southern than in the northern part of Wood's vein is probably to be connected with a pre-glacial topography the nature of which is not now quite clear. The southern part of the depression which marks the fault occupied by Wood's vein is comparatively deep and narrow and it seems feasible to suppose that protection against corrosion was given by the hard steep walls of greenstone on either side, especially after the depression had been filled with débris by the initial advance of the ice.

In connection with the problem, there is another factor which should not be ignored. The southern part of the Keeley property is the locus of numerous faults branching from the main Wood's fault, which continues southward to and beyond Trout Lake. The surface of the lake is quite 200 feet below that of

[7] Henry H. Knox, "Superficial Alterations of Ore-Deposits," ECON. GEOL., vol. XVII., p. 655 et seq.

[8] J. B. Tyrrell, "Pre-Glacial Oxidation in Northern Ontario," ECON. GEOL., vol. XVIII., p. 296; also C. W. Knight, *loc. cit.*, p. 766.

the Wood's fault at the Keeley while the deepest part of the bottom of the lake is probably 250 feet still lower. The section of the fault in question is the main water-carrier of the Keeley at the present time, and it seems reasonable to presume that in preglacial times, if not more recently, there was a subterranean connection along the fault to Trout Lake, the meteoric water descending to comparatively great depths and rising under head in the lake basin.

EFFECT OF THE DEEP OXIDATION.

Wherever "ferric" conditions exist in the Keeley mine and notably in the southern part of Wood's vein, the vein-material has been almost wholly deprived of the silver content, which it may safely be assumed was originally present in view of the general character of the vein-material under comparable structural conditions in the unaltered parts of the mine. The unoxidized vein-material is generally relatively high in iron sulphides as previously explained. With their oxidation, sulphuric acid, and acid-ferric sulphate would be liberated, dissolving silver as sulphate according to the reactions detailed by Emmons.[9]

The essential equation bearing upon the argument under discussion is as follows:

$$FeS_2 + 7O + H_2O = FeSO_4 + H_2SO_4.$$

Ferrous sulphate in the presence of free oxygen would be oxidized to ferric sulphate, or to ferric sulphate and ferric hydroxide:

$$6FeSO_4 + 3O + 3H_2O = 2Fe_2(SO_4)_3 + 2Fe(OH)3.$$

As Emmons indicates, "mineral waters in sulphide ores near the surface are solutions of sulphuric acid and ferric sulphate. With increase of depth the acidity of such solutions decreases and ferrous sulphate accumulates."

Ferrous sulphate precipitates silver from sulphate solutions according to the following reaction:[10]

[9] "The Enrichment of Ore Deposits," Bull. 625, U. S. Geol. Surv., p. 106 et seq., and p. 252 et seq.

[10] W. H. Emmons, *loc. cit.*, page 270.

$$Ag_2SO_4 + 2FeSO_4 = 2Ag + Fe_2(SO_4)3.$$

Recognition of the solubility of silver under acid ferric sulphate conditions and its deposition under ferrous sulphate serves to explain the peculiar features of the altered section of the veins of the property previously outlined, more especially the absence of silver in "ferric" vein-material and its presence in "ferrous."

Palmer and Bastin [11] have shown how smaltite is a moderate precipitant for silver, but as smaltite is more readily soluble by meteoric waters than silver it could scarcely be expected to precipitate the latter element except below or on the edge of the zone of alteration. The edge of the zone of alteration seems to be apparent in No. 26 vein, where it seems probable smaltite may have been partly responsible for the deposition of silver, dissolved from the leached zone above.

What little microscopic work that has been done on the Keeley ores serve to corroborate the evidence, derived from analyses and gained more particularly from the careful accumulation of geological data, that a substantial part of the silver in certain of the rich shoots of the Keeley is secondary and derived from the impoverished highly oxidized upper portions of the veins. In making this statement the writer does not attempt to combat the now generally accepted theory as to the hypogene origin of the primary ore in veins of the cobalt type, to which those of the Keeley mine clearly belong. Much of the Keeley ore is probably purely primary and has gained no enrichment from descending solutions, but in other parts of the mine, where, indeed, the richest ore has been found, there would seem no reason to doubt the existence of widespread changes, which have materially altered and enhanced the value of the primary mineralization.

[11] Chase Palmer and Edson S. Bastin, "Metallic Minerals as Precipitants of Silver and Gold," ECON. GEOL., vol. VIII., No. 2, p. 152.

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EDITORIAL

SPECULARITE CHARACTERISTIC OF VEINS AT SHALLOW DEPTHS.

It is well known that high temperature ore deposits or deposits formed in the deep vein zone,[1] such as contact metamorphic deposits, or pneumatolytic veins, are often characterized by the presence of specularite. The occurrence of this mineral in deposits formed at moderate and shallow depths, however, has been reported from time to time, as in the peculiar tin veins in northern Nevada,[2] and others. In Japan, primary specularite is not infrequently found in veins formed by hot solutions in the Tertiary sedimentaries and lavas, so that it may be regarded as characteristic of veins formed at shallow depths. Some typical examples may serve to illustrate this:

The copper veins of the Osaruzawa mine, the Arakawa mine, the Furokura mine, and many others in northeastern Japan are found in Tertiary sedimentary rocks covered and intruded by tuff, rhyolite, and pyroxene andesite, the last rock being often altered into propylite. In most of these veins, the principal mineralization is represented by a mineral association—chlorite + quartz + pyrite + chalcopyrite with varying amounts of galena, zincblende, and calcite. It is noteworthy that the copper veins in question are often intricately cut by veins and veinlets, consisting chiefly of quartz and micaceous specularite with insignificant quantities of pyrite and chalcopyrite and which may show a crustified structure. The specularite is decidedly primary in origin and the veins containing it represent evidently the last phase of the copper vein formation, genetically connected with the late Tertiary volcanic activity.

[1] W. Lindgren, "The Relation of Ore Deposition to Physical Conditions," Econ. Geol., vol. II. (1907), pp. 105–127.

W. H. Emmons, "The Principles of Economic Geology," 1918, pp. 49–71.

[2] A. Knopf, "Wood Tin in the Tertiary Rhyolite of Northern Nevada," Econ. Geol., vol. XI. (1916), pp. 652–661.

The copper veins of the Omori mine offer another example of the occurrence of specularite. The mine lies in western Japan near the coast of the Sea of Japan and is accessible by the Sanin Railway. This district is composed of Tertiary sandstone and tuff covered by a thick flow of biotite hornblende andesite and its agglomerate. Many parallel veins occur chiefly in the propylitized andesite and tuff. They are chalcopyrite-quartz veins containing varying amounts of pyrite, zincblende, galena, and other sulphides with a little gold and silver, often cut by siderite veins and siderite-specularite veins. Barite which is also an important gangue mineral is still later in age. The specularite is found more abundantly on deeper levels and is decidedly primary in origin, but it is evidently of a later phase of mineralization.

Still another instructive example is the ore deposit of the Aoné mine at the foot of the volcano of Zaô in northeastern Japan. The deposit consists of irregularly crossing veins, *i.e.*, a network of veins, in a Tertiary green shale which is usually bleached and silicified, the exposed mineralized zone being several meters in width. Micaceous specularite, sometimes in a powdery aggregate, in association with quartz fills the veins. Pyrite is sporadically present and apparently increases in amount downwards. This deposit was once worked for iron ore, naturally without success. This peculiar deposit is genetically related to the extrusion and intrusion of rhyolite in this district, a large dike being found within a distance less than ½ km. from the deposit. The specularite is decidedly of primary deposition from hot solutions at a very shallow depth.

In this connection it is interesting to refer to Rastall's recent paper[3] on metallogenetic zones in which he generalizes Cronshaw's investigation of many typical lodes in Britain. It is here emphasized that in Japan as in Britain, iron ores, especially specularite and siderite, are characteristic associates of the veins belonging to the remotest or last metallization in relation to a center of igneous activity.

TAKEO KATO.

[3] R. H. Rastall, "Metallogenetic Zones," ECON. GEOL., vol. XVII. (1923), pp. 108–121.

DISCUSSION AND INFORMAL COMMUNICATIONS

THE MUTUE FIDES—STAVOREN TIN FIELDS.

Sir: May I be permitted to make a few remarks on the review by Mr. Frank L. Hess, in the January-February issue of ECONOMIC GEOLOGY, of my memoir on the above-named fields.

Mr. Hess's criticism in regard to the use of the term xenolith as synonymous with inclusion is perhaps somewhat hard to defend. In using it I err, if at all, in good company, *e.g.*, Sollas (its coiner), Harker, and Daly, to mention only a few names. As regards Mr. Hess's comments on the subject matter of the memoir, it might be asked whether it is not going too far to impeach the accuracy of the deductions of a fellow worker in a distant field because they do not happen to agree with his own from deposits in the formation of which totally different physical and chemical conditions may have obtained.

To take, for instance, the replacement by cassiterite of quartz in preference to feldspar, which causes him misgivings; this is not only a common phenomenon in the area under review, but is the rule in many of the deposits of the more important Waterberg Tin Fields. Here Kynaston and Mellor[1] actually came across an instance where cassiterite had placed the quartz of a graphic granite while the feldspar was not in any way altered; so that the original graphic structure is represented by narrow interpenetrations of feldspar and cassiterite.

Then there is the replacement of wolframite by scheelite. It is well established that scheelite can replace wolframite, and wolframite scheelite, the latter process being, as pointed out in Dana's "System of Mineralogy," the more common. In the

[1] Memoir No. 4, Geological Survey, Transvaal, 1909, p. 64.

specimens described by the writer irregular crystals of wolframite, having the general habit and the cleavage of that mineral, are found on examination to consist partly of scheelite that has clearly developed at the expense of the wolframite from interlacing cracks and fissures. Could a more perfect illustration of replacement be desired?

Mr. Hess is in error in attributing to me the statement (p. 91) that wolframite replaces cassiterite. What I say is that wolframite was found in one instance to be moulded on crystal cassiterite replacing an older quartz crystal.

Turning to some of the other criticisms, it is extraordinarily difficult to follow Mr. Hess when he suggests that vugs in pegmatite lined with crystals of quartz and fresh feldspar are of the nature of solution cavities.

I must also protest against the statement that I do not clearly recognize the relationship of the pipes to the pegmatite "eyes," because on page 119 I say of the new "B.2" working: "Though generally referred to as a pipe this is not a pipe at all, but consists of a number of superimposed replacement deposits in irregular bodies and eyes of pegmatite aligned along a vertical fissure." To the reader it will be obvious that the term *pipe* is here used in its morphological sense.

Finally, if it is so difficult to follow me in my deductions, how is it that the theory of origin of the deposits briefly put forward by Mr. Hess is identical with that elaborated on pages 114 and 164 of the memoir. The only difference, indeed, between our views is that Mr. Hess regards the pegmatites—presumably those in the granophyre are referred to—as replacements of the country rock, while I could not find sufficient evidence for this assumption. Of the majority of the bodies of pegmatite in the granite it can be stated definitely that they are not replacements.

Mr. Hess will, I am sure, agree with me that in the study of ore deposits the only course to pursue is to make accurate observations and to base one's conclusions on those observations, and not upon what has or has not been observed before. If some of the phenomena presented by the South African tin de-

posits are at variance with the views of what tin deposits the world over should be this is unfortunate, and may perhaps indicate that previous ideas stand in need of revision.

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PRIMARY CHALCOCITE: BRISTOL, CONN.

Sir: Your important paper on primary chalcocite at Bristol, Connecticut (ECON. GEOL., vol. XVIII., March, 1923), has led me to submit the following data on the Frigido copper mine of Italy, which strikingly resembles the Bristol deposit.

The Frigido mine lies near the town of Massa, Tuscany, halfway between Pisa and Spezia. The country rock is a crystalline schist of Triassic age, stained dark brown by iron oxide. Tourmaline and mica are abundant, and quartz and garnet occur along the surfaces of foliation. Intrusive rocks are not found near the mine, and the ore deposit is visible only in a vein along the steeply dipping foliation planes.

The principal minerals found are: siderite, by far the most important, followed in order by chalcopyrite, chalcocite, galena, zinc, blende and silver. Gold is also present, but has no commercial importance. The vein containing the ores is from one to six feet thick; siderite composes 70 per cent., quartz 25 per cent., and the remaining 5 per cent. is copper—chalcopyrite and chalcocite.

Distribution of the Ores.—Siderite is found lining the unaltered wall rock, and contains a white amorphous quartz within which chalcopyrite is disseminated, intermingled here and there with chalcocite. It is noteworthy that the chalcocite occurs where the chalcopyrite is most abundant.

Investigation of this ore deposit indicates, as at Bristol, that chalcocite was the last mineral to be formed. Its hypogene origin seems to be proved because it is interlocked with quartz where chalcopyrite is lacking. In the lower part of the deposit

no secondary alteration has occurred and the minerals appear fresh; in such zones both chalcocite and chalcopyrite are more abundant.

Ore Formation.—Aqueous solutions of carbonated waters, travelling at a somewhat low temperature (below 91° C.[1] if we consider the occurrence of orthorhombic chalcocite crystals), have taken in solution as iron carbonate part of the iron disseminated in the crystalline schists of the country rock, making it the predominant feature of the metallizing solutions. Silicon, copper, and other elements were also probably taken from the country rock, but were deposited after the greater part of the siderite, which explains the purity of the ore.

After the carbonated waters had transformed all the iron in solution to siderite, a part of the iron may have been carried in solution as ferric oxide, which seems to have been the case in both the Bristol and Frigido deposits. This ferric oxide may have oxidized part of the sulphur in the solutions, resulting in the hypogene chalcocite.

A careful observation of the distribution of different ores through the deposit seems to indicate that the iron of the chalcopyrite is proportional to the chalcocite.

LEONE CORNELIO SAGUI.

FABRIANO,
ITALY.

[1] Posnjak, E., E. T. Allen, and H. E. Merwin, "The Sulphides of Copper," ECON. GEOL., Vol. X., 1915, pp. 491–535.

REVIEWS

The Petrology of the Sedimentary Rocks. By F. H. HATCH AND R. H. RASTALL. London: George Allen and Unwin Ltd., Revised Edition, 1923, pp. xv + 368, 5 × 7½ inches.

This is the second volume of the authors' "Text Book of Petrology" and in form and contents is distinctly of the textbook type. It is divided into two parts, the first of 150 pages (less than half the book) dealing with the types of sedimentary rock and their origin; the second of 200 pages with their metamorphism in its broadest sense.

The first part is divided into four chapters, of which the first serves as a general introduction presenting a brief description of sediments according to facies of deposition—marine, estuarine, and continental. For the practical purposes of discussion this "theoretically excellent" classification is rejected in favor of a more convenient subdivision into clastic, chemical, and organic, each of which is the subject of one of the three subsequent chapters. In the consideration of metamorphism the authors accept in a general way Van Hise's classification of the zones of metamorphism but for practical purposes use what they call a "geological basis" (p. 159), devoting a chapter each to cementation and metasomatism, to contact metamorphism, to regional metamorphism, and to weathering.

According to the authors the new matter added to this second edition includes paragraphs on the influence of climate on sedimentation, chalk, crinoidal limestone, oölite, marl, brockram, gypsum, radiolarian chert, loess, the iron ores of Lake Superior, etc., the potash deposits of Elsass and of Spain, and crush conglomerates.

That some of these subjects should have been omitted from the first edition not only seems a grave defect but raises a doubt as to the thoroughness of this new edition. The list appears to be significant of the haphazard character of the book, due, it seems, as much to defective knowledge and scholarship in the field with which it deals as to deliberate choice.

The authors' knowledge of the relevant literature appears scattered and accidental, restricted largely to that of their own country and even there conspicuous throughout for the antiquity of many of their references. A compensation for this defect is that the geology and geological literature of South Africa (of whose geological society the senior author is a past-president) is unusually well represented and many special features of

this region, not brought up in most general treatises, are thus worked into the reader's view of the subject. It is evident, however, that the authors have made no attempt to keep up with developments, and in a field in which literature is accumulating so rapidly, this failure detracts greatly from the value of their production. For instance, they refer on page 23 to a paper published in 1852 dealing with the transportation of debris by running water, ignoring G. K. Gilbert's elaborate paper of 1914 and on page 205 to Prestswich's presentation in his textbook of geology of the theory of formation of fine-grained limestones by precipitation, ignoring the recent work of Vaughan, Drew, and associates, though Drew's work is known to them. This superficial knowledge or inadequate use of even the work with which they are acquainted, is illustrated again on pages 218 and 219 in their discussion of oölitic iron ore, where they refer to English publications of 1879 and 1886, ignoring Cayeux's important and elaborate work, still in progress, in support of the hypothesis that the iron is of syngenetic or nearly syngenetic submarine origin, not derived by weathering. Yet they seem to know of much of Cayeux's work.

If the authors' knowledge of the subject is uneven and haphazard there are nevertheless parts which appear to be treated adequately for a textbook of this kind and which make one regret that the entire book was not brought up to this standard. Such are the discussion of coral rock on page 130 and following pages, with its generous though still rather hit-or-miss list of references and its emphasis on the large contribution of calcareous algæ to reef limestones; and the discussion of coal, covering pages 136 to 149.

A reviewer always runs a chance of being unjust and exaggerating the importance of his own particular interests in pointing out topics omitted, since in a small volume like this much must necessarily be left out. Nevertheless, it does seem, for instance, that the changes taking place in salt deposits and the metamorphism of these deposits might deserve some consideration not only as a part of the discussion of these deposits but also for their bearing on the general problem of metamorphism to which the authors give so much space. Even in the treatment of the more common metamorphic products their work is very incomplete. In the discussion of contact metamorphism, for instance, the emphasis is laid on that kind in which the composition of the sedimentary rock is not changed; little space is given to metasomatic changes and none to the replacements of limestone by garnet, epidote, and other lime silicates, with, in places, ores of copper, zinc, iron, tungsten, etc.

Perhaps less excusable than either of the above defects are the careless, incomplete, or erroneous statements, made often with great finality, which occur throughout the book. The explanation offered by the au-

thors (p. 39) may "easily" account for the relative rounding of sand by wind and by water, but there would seem to be still enough uncertainty to demand a mention, at least, of such facts as relative densities and viscosities of the transporting media involved (one of which, incidentally, is recognized on page 53). The explanation of colloidal precipitation on page 73 may be predominantly true, but those most familiar with the subject do not seem to share the authors' certainty about some of the factors involved. One would like to know from what source the authors got the idea that circulation through the Straits of Gibraltar (p. 94) is in only one direction. This might be so significant, if true, that a misstatement about it is hard to excuse. Even more pernicious is the statement on page 169 that porosity is greatest in coarse sediments, because that statement is contrary not only to facts but also to the fundamental principles of the subject. If, as explained on pages 298 and 299, silica tends under regional metamorphism to form quartz because that is the densest form, how does "very similar reasoning" lead to the conclusion that for "pure calcareous rocks—the only possible change under conditions of high pressure is recrystallization—as calcite" rather than the denser aragonite. And finally what sort of reasoning habits are developed in a student by such meaningless use of words as on page 296 in the statement "The minerals characteristic of this zone are therefore complex, stable," etc. What meaning has stability apart from a consideration of the range of conditions under which it is maintained?

By itself a thoughtless statement like this might not be significant but I fear it is typical of the spirit in which the entire book is written. One who has never been a teacher has perhaps no right to speak on pedagogical matters, yet I should like to protest against the final, conclusive, often arbitrary way in which many of the topics in this book are treated. Throughout, the subject matter is presented as a mass of well-established conclusions often (as illustrated by some conspicuous examples above) when the facts do not in the least warrant such assurance. Yet it would seem that for a student about to enter the field it is the problems that are of real importance and that will best serve to awake in him an eagerness to do his share in adding to the general store of knowledge. The scantiness of references to the literature is in fact only a manifestation of this spirit of finality on the part of the authors, and the omissions may also be largely dictated by it. Perhaps, as some pedagogical friends have suggested, the only way to make facts assimilable is to present them categorically, but then that is to assume that it is important to assimilate facts—even when they are not true. At least, if we insist on this method of education we should not complain if the process of converting graduates of our universities into geologists consists largely in curing them of their education.

In conclusion, the book has some value for what it contributes, especially from the authors' personal experience, to subjects that are not yet adequately covered by any book known to me. For that reason schools of geology and workers specially interested in the field will refer to it. While from the necessities of its size the book is elementary, yet even within its scope it is incomplete.

MARCUS I. GOLDMAN.

Copper Ores, Monograph of the Imperial Institute. By ROBERT ALLEN, pub. John Murray, London.

The book is frankly a compilation of the readily available statistical data on copper industry with a short discussion of the geographical occurrence and the broad geologic features of the principal copper deposits of the world and a summary of the history of the industry.

The book contains nothing new but is the only existing publication in which the extensive literature on copper is summarized and arranged in an orderly manner.

The statistical data are taken from reliable sources and present a useful summary but the important questions of quantity and political and commercial control of reserves are unsatisfactorily discussed. This part of the book is taken almost entirely from F. W. Paine's "Geographical and Financial Control of the World's Copper Mines" which is essentially correct from the point of view of production but is inaccurate as a general guide to potential production and competition.

The discussion of costs of production is limited to a part of the American production for a few years and is but a very rough guide as to costs in general.

The discussion of the reserves, costs of production, etc., of principal Russian, Spanish, and African mines and deposits is disappointing in view of the fact that most of the important ones are owned or operated by companies whose headquarters are in London.

On the whole the book should be useful and valuable to all who are interested in the industry but who have not made an extensive study of its broader aspects. It is noteworthy for the conservative note which prevails throughout and for the frank full credit given all sources of information.

H. A. C. JENISON.

SOCIETY OF ECONOMIC GEOLOGISTS

ANNUAL MEETING

The annual meeting of the Society will be held in Washington, D. C., December 27-29, 1923, in conjunction with the annual meeting of the Geological Society of America; but no separate technical program will be offered at that time. It is hoped that this decision will help to relieve the growing congestion of schedules in recent years and will reduce to a minimum the necessity for conflicting sessions.

Detailed announcement and program, including such joint sessions as may be arranged, will be sent to the members well in advance of the date of meeting.

INTERNATIONAL STRENGTH

Perhaps few members of the Society are aware of the fact that out of the total present membership of 175, 25 members, or 14.3 per cent., reside outside of the United States. Canada leads with 8 members; Australia has 3; Belgium, England, Mexico, and South Africa, 2 each; while Holland, India, Italy, Japan, New Zealand, Norway, South Africa, and Sweden are represented by one member each. From the beginning it was foreseen by its most active members that the interests, activities, and membership of the new organization would become international; and the alternative titles from which the name of the Society was originally chosen were all so designed as to avoid the suggestion of national limitations. It is gratifying to find that, in so short a time, this ideal is being realized to a marked degree; and further development along this line may be confidently anticipated, under the guidance and advice of the present membership in the various countries.

SCIENTIFIC NOTES AND NEWS

George Otis Smith has been reappointed Director of the U. S. Geological Survey.

W. H. Emmons has returned to the University of Minnesota from the Argentine Republic, where he spent the summer making geological surveys of mining districts.

Herbert E. Gregory has returned from Honolulu and will continue his duties at Yale University until February, when he will again return to Honolulu. During the summer he attended the Pan-Pacific Congress in Australia.

W. O. Hotchkiss, State Geologist of Wisconsin, has been directing a magnetic and geological survey of the pre-Cambrian area of that state, which is in the immediate charge of H. R. Aldrich.

Sydney H. Ball is in Europe.

L. W. Stephenson is in Venezuela, making geological observations in the petroleum region of that country.

James Bartlett has resigned his position as Provincial Inspector of Mines in charge of the Sudbury district of Ontario to become associated with the Coniagas mine in Cobalt.

C. W. Gudgeon has left Tasmania to take up his residence as a Consulting Engineer in Melbourne, Victoria.

D. B. Reger is preparing a report on Mercer, eastern Summers, and Monroe counties for the West Virginia Geological Survey.

William Sloan, Minister of Mines of British Columbia, recently visited the underground workings of a number of properties in the Slocan district.

W. M. Agar has been appointed instructor in Mineralogy and Economic Geology at Yale University.

R. W. Brock has returned to his duties at the University of British Columbia after spending the summer in China and Australia.

H. W. Turner is examining mines in the state of San Luis Potosi, Mexico.

Richard W. Smith, Assistant Geologist, Tennessee Division of Geology, was engaged this summer in field work on the preparation of a detailed bulletin on the phosphate deposits of Tennessee.

Cyril W. Knight, formerly associate geologist of the Ontario Department of Mines, has left the government service to take a position with the Nipissing Mining Co., Cobalt, Ontario.

M. H. Caron, mining and metallurgical engineer, recently arrived for another visit to this country from Batavia, Dutch East Indies.

J. Morrow Campbell is in London on short leave from Burma

The members of the field parties that were making surveys of the Petroleum Reserve in the Point Barrow region, Alaska, under the leadership of Sidney Paige, have returned to Washington.

Chester W. Purington and his two children, with their governess, lost their lives in the earthquake in Japan. Mrs. Purington reached Kobe in safety. Mr. Purington was in Tokio at the time of his death.